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ART. I. *A Biographical Memoir of Samuel Bard, M. D. LL. D.*
late President of the College of Physicians and Surgeons of the
University of New-York; with a Critique upon his writings.
Read before the New York Historical Society, August 14th,
1821. By Henry W. Ducachet, M. D.

BIOGRAPHY has very justly been pronounced one of the most nice and difficult kinds of composition. It requires a judgment with which few are endowed, to make a judicious selection of the incidents of a life; to arrange facts in a striking order; and to narrate them in an interesting manner. The biographer is embarrassed with the multitude of materials which crowd upon him: he is at a loss what to choose, and what to omit; when to be brief, and when to amplify; and finds it difficult to avoid the extremes of a wearisome minuteness, and a too rapid recital. But it is still more difficult to give a true and faithful delineation of the character he is describing; to preserve a just moderation in eulogizing its excellencies, and a necessary impartiality in touching its defects. No wonder, then, that so few excel in biography; and that lives are so generally either barren and unprofitable records of dates and facts, or pompous and extravagant encomiums.

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Without any pretensions to the rare talent for biography, I propose to give a sketch of the life and character of Dr. Bard,—a man whose life is worthy of being recorded for the admiration and example of posterity.

Dr. Samuel Bard was born in Philadelphia on the 1st of April 1742. His grand-father had been driven to this country by the memorable revocation of the edict of Nantes; and, settling in Burlington, New-Jersey, became one of the judges of the supreme court of that province. His father was Dr. John Bard, afterwards a distinguished physician of New-York, and memorable for being the first person who performed a dissection and taught anatomy by demonstration on this side of the Atlantic.* His mother was a Miss Vallean, a neice of the highly respectable Dr. Kearsley of Philadelphia, and likewise a descendant of the Protestant refugees. At the time of Dr. Bard's birth his father was practising his profession in Philadelphia; but at the urgent solicitation of Dr. Franklin, he shortly after removed with his family to New-York, in consequence of the death of several eminent physicians in the epidemic yellow fever which desolated this city in 1741 and 42. Dr. Bard received the rudiments of classical education in New-York, at a respectable grammar-school under the direction of a Mr. Smith; and at the age of 14 years entered King's College under the private pupilage of Dr. Cutting, at that time professor of languages, and during the presidency of Dr. Samuel Johnson. While at college he gave some attention to the study of medicine; and afterwards regularly devoted himself to the profession under the auspices of his father. About this time he imbibed his taste for botany from Miss Jane Colden,† daughter of the then lieutenant-governor of the province. She instructed him in the elements of that interesting science during his occasional visits to the family; and he repaid her attentions by drawing and colouring plants and flowers for

* In 1750 Dr. John Bard dissected the body of Hermannus Carrol, who had been executed for murder; and injected the blood-vessels for the use of his pupils.

† This lady was a correspondent of the celebrated Linnaeus, and was honoured by him in having a plant which she first described called *Coldenia*.

her.* In the fall of 1760, he sailed for Europe; but being captured by a French privateer he was taken to Bayonne, and confined six months in the castle. Upon his release in the spring of 1761, he immediately proceeded to London. He was now, at the recommendation of Dr. Fothergill, received into St. Thomas' hospital as the assistant of Dr. Russel, the celebrated author of the history of Aleppo; and continued in that capacity until his departure for Edinburgh, enjoying in the mean time the instructions of Mr. Else the surgeon, of Dr. Grieve the translator of Celsus, and of Dr. Akenside the poet. At the time of Dr. Bard's arrival in Edinburgh, that celebrated school was in the meridian of its glory. Dr. Robertson the historian was its principal; and Rutherford, Whytt, Cullen, the Munros, the elder Gregory, and Hope, its professors. During his attendance at Edinburgh, he acquired the reputation of an ingenious and indefatigable student; and was considered one of the most intelligent Americans who had yet visited that celebrated seat of learning. He was particularly distinguished as a classical scholar, having made great proficiency in the languages under the tuition of the celebrated, but unfortunate Dr. Brown. He graduated in 1765, after having defended and published an inaugural essay "*de viribus opii*;" and left Edinburgh loaded with honour, in consequence of having obtained the prize offered by Dr. Hope for the best Herbarium of the indigenous vegetables of Scotland.† He was the room-mate of Dr. Saunders the author of the valuable treatise on the liver; and the fellow-student of Dr. Withering, the author of the Medical Botany; of Dr. Percival, the writer on Medical Ethicks; of Dr. John Morgan, one of the founders of the University of Pennsylvania; of Dr. Sims, the president of the Medical Society of London; of Dr. Bostock, the father of the

* This taste for botany and drawing he always retained. A pretty specimen of both may be seen in one of the volumes of the Transactions of the Royal Society. It is a representation of the first plant of the rhubarb which grew in the botanic garden of Edinburgh, and accompanies Dr. Hope's paper upon that subject. He also furnished the plan for Trinity Church, New-York.

† This valuable collection contains duplicates of upwards of 500 plants, and is still, I believe, in excellent preservation.

present chemist; of Carmichael Smyth, who has distinguished himself by the introduction of acid fumigations to destroy infection; of Dr. Andrew Duncan, senr. afterwards professor at Edinburgh; of Dr. Haygarth, who has immortalized his name by his professional exertions in the cause of humanity; of Professor Parsons, now of Oxford; and of Sir Lucas Pepys, late physician to the king of Great Britain.

In 1765 he returned to his native country, and commenced the practice of medicine in New-York in connexion with his father. Dr. Bard was thus very soon introduced into the extensive and respectable circle of practice which he retained till the time of his removal from the city, and in which he acquired a popularity and reputation which seldom fall to the lot of a physician in this country. He seldom practised surgery, having a natural sensitiveness of temperament which revolted at the dismaying duties of an operator.

About this time very great attention was excited on the subject of medical education in the provinces. The great increase of wealth and population, the degraded state of the profession in consequence of the extensive prevalence of quackery, the inconveniences and expense of a journey to Europe, and the recent accessions of talent and respectability to the medical corps, determined a number of public-spirited gentlemen of Philadelphia to establish a school for medical instruction in that city. Under the auspices of Shippen, Morgan, and Kuhn, a highly respectable college was organized. New-York soon followed this worthy example; and in 1768, a similar establishment was opened in this city. Dr. Samuel Bard was immediately appointed to teach the theory and practice of physic, the most important branch of all.*

At the first commencement held by the new college in 1769, Dr. Bard delivered the address to the graduates. Of the discourse he pronounced on that occasion, I shall speak more particularly hereafter. Suffice it to observe for the present, that it was the means of establishing the New-York hospital. The

* Dr. Clossey was chosen professor of anatomy; Dr. John Jones of surgery; Dr. Middleton of physiology and pathology; Dr. Smith of chemistry and materia medica; and Dr. Tennant of midwifery.

courses of instruction thus distributed were continued regularly, and with unexpected success, for several years; but the institution so promising of good was interrupted, and finally destroyed by the revolutionary war.

On the commencement of hostilities in 1776, Dr. Bard's political principles being odious to the generality of the community, he thought it prudent to retire to Shrewsbury, New-Jersey. He there occupied himself in preparing salt; but not succeeding to his satisfaction, and being unable to support his family comfortably, he returned to New-York on its being taken possession of by the British troops. He immediately regained the lucrative practice he had left; and was so successful in business, that at the end of the war he possessed a handsome independence. The high character which Dr. Bard maintained at this period, cannot be better shewn than by the fact, that, notwithstanding political differences, (and party-spirit was the ruling principle of the day,) he was the family physician of general Washington during his residence in New-York.

After several abortive attempts by the regents of the university to revive the medical school on the restoration of peace, the trustees of Columbia College resolved to place it upon a permanent foundation, by annexing the faculty of physic to that institution in 1792. Dr. Bard was continued as the professor of the theory and practice of medicine, and was appointed dean of the faculty. At this time he was still engaged in a very extensive practice; and was zealously occupied in teaching the different branches of the profession to a large number of private pupils. The professorship of natural philosophy in Columbia College being vacant for some time before the arrival of Dr. Kemp, Dr. Bard, notwithstanding his multiplied avocations of a public and private nature, undertook to supply this deficiency in the course of instruction. His exertions were chiefly instrumental in the establishment of the city library, and of the New-York Dispensary; and were always conspicuous in plans for the advancement of science, and the promotion of human happiness.

In the year 1795 he took Dr. Hosack into partnership; and in 1798 retired into the country, leaving that gentleman successor

to his practice. It is proper to state, that, although Dr. Bard had now resigned forever the cares of professional life, his sense of duty would not permit him to be absent from the city during the dreadful epidemic of '98; but prompted him to the scene of desolation and terror, to administer assistance to his suffering fellow-citizens. It was not until he had been disqualified by an attack of the epidemic for the arduous labours which a physician must undergo in a season of pestilence, that he could be urged again to retire into the country.

The legislature of the state had passed an act in 1791 for the establishment of a college of physicians and surgeons in the city of New-York; but the regents of the university did not avail themselves of the power thus granted until the year 1807. The medical schools of Columbia College and of the University, by an erroneous policy which had nearly proved ruinous to both, were suffered to remain rival institutions until the year 1813. It was now discovered that it was impossible for two medical schools to flourish in the same city: and, accordingly, it was determined to organize an institution entirely new, which should combine the talents and the learning of the rival colleges. The present college of physicians and surgeons of this city is the offspring of this judicious coalition. Dr. Bard was appointed its first president, and retained the office until his death.*

During his residence in the country, he zealously engaged in the pursuits of agriculture; and in 1806 was elected president of the Agricultural Society of Dutchess county, which he had been chiefly instrumental in forming. He was the founder of the neat little church at Hyde-Park in the neighbourhood of his residence, and the principal contributor to the expenses of its erection. From this parish he was repeatedly delegated as a member of the public council of the Protestant Episcopal church in the state of New-York.

In the year 1811, he was elected an honorary member of the college of physicians of Philadelphia; and in 1816, the degree of Doctor of Laws was conferred upon him by Princeton college.

* Dr. Bard had two years before been elected President of the original College of Physicians and Surgeons.

Dr. Bard was never ambitious of such distinctions. He never sought them by courting the correspondence of distinguished men abroad, or by assuming a fictitious importance by pomp and parade at home.

He lived to the advanced age of 79 years. In the latter years of his life he was afflicted with several severe attacks of a stricture of the œsophagus, which greatly increased the bodily infirmities incident to old age. But to his last days he retained the perfection and vigour of his mind. Sensible of his approaching end, he had made it a business to prepare for death. And after arranging his temporal concerns to his satisfaction, and spending his last hours in devotional exercises, he departed this life after a few hours illness of a pleurisy, on the 25th of May last, looking with the hope of a christian for a christian's reward.*

In whatever light the CHARACTER of Dr. Bard may be viewed, it must elicit admiration, and exhibit itself in the commanding attitude of a model. Do we consider him as a *professional* man? We find him among the first physicians whom his country has produced. Dr. Bard was not one of those physicians who content themselves with the elementary knowledge they acquire in their academic studies, and rest satisfied with the slender attainments which qualify them to maintain a reputable intercourse with their brethren. He viewed medicine as a deep and extensive science, embracing almost every department of human learning; continually enriching herself with the accumulating experience of ages; and requiring of her votaries patient, laborious, and unceasing study. Accordingly, we see him at an early age engaging in the study of medicine with an assiduity of which youth is seldom capable; continuing his investigations with an ardour which the

* The particulars of Dr. Bard's life have been communicated to me by his son Mr. William Bard, and his son-in-law, the Rev. Professor M'Vickar of Columbia college. Dr. Hosack, and Dr. J. W. Francis have also favoured me with some facts of an interesting nature. In acknowledging my obligation to these gentlemen, I would beg leave to tender them my thanks for the promptitude and courtesy with which they answered my inquiries.

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soberness and occupations of maturer age were not able to subdue; and manifesting to the last his attachment to his favourite study by unremitted application, at an age when the delights of science have commonly lost their enchanting power, and the feebleness and decrepitude of the body have destroyed the energy and vigour of the mind.

But Dr. Bard did not pursue the science of medicine for the selfish gratification which study affords; nor did he wish to become learned merely for the sake of being so. He had a more honourable and useful purpose to stimulate him to diligence. It was to qualify himself to discharge the high and serious responsibilities of his professional station, that he gave his days and nights to laborious industry. It was that he might become a skilful and sagacious practitioner, that he devoted to the scientific departments of his profession the intervals of leisure which he snatched from the hurry and employments of an extensive practice. Nor were his labours unsuccessful. He did attain the character which was the object of his noble ambition. The proverbial sagacity and skill of Dr. Bard as a practitioner of medicine, are to this day the theme of popular admiration. Perhaps there never was a medical man in the city of New-York, so universally known, so much beloved and esteemed as a practitioner. Indeed, so astonishingly popular was he at one time, that, notwithstanding the number of worthies who flourished cotemporaneously in the same city, he was called to almost every person who was taken sick. It was unfashionable to be sick without being visited by Dr. Bard. Nor did he acquire his wonderful popularity and his extensive practice, merely by the reputation he had obtained as a learned physician and a skilful practitioner. Learning and skill, however desirable they may be, are very properly considered by the intelligent and worthy portion of the public, as insufficient to entitle to their patronage a physician who is destitute of those sensibilities which are the highest ornaments of our nature, or who seems to have no other view of his profession than to make it subservient to the accumulation of wealth. Dr. Bard possessed qualities which, had he been but an ordinary man, would have secured to him the attachment and esteem of the community, and

the confidence of his professional brethren. He was remarked for his assiduous attention to the sick, for his humanity to the poor,* and his tenderness to all. He was proverbial for the conscientious, and faithful, and liberal discharge of all his obligations to his patients. And in his intercourse with his brethren he acquired the unanimous approbation of the profession as a gentleman of undeviating integrity, and of high and delicate honour.

Do we view him as a *public functionary*, discharging the important offices of a teacher of medicine; undergoing the hazardous duties of the health office; assuming the high and honourable post of President of the College of Physicians? In all these capacities we find him an example of inimitable fidelity to his trust, equalled only by the ability with which his duties were discharged.

As a professor Dr. Bard deservedly ranks among the first whom this country has produced. Versed in the department which it was his province to teach, and possessing an admirable talent for instruction, we find him communicating to his pupils the lessons of wisdom and experience, in a style of eloquence, not vehement indeed or powerful, but simple, dignified, and interesting. Aware of the influence which a public teacher possesses over the minds of his pupils, he availed himself of his popularity and of the opportunities afforded by his office, (not to pervert the moral and honourable principles of the youth committed to his care; not to destroy the ingenuous sensibilities of their hearts by disseminating in his prælections the principles of a licentious philosophy, by lewd and indecent illustrations, or by anecdotes calculated rather to excite unhallowed passions than to convey any useful instruction, but) to inculcate the principles of virtue and religion, to rebuke and reclaim the dangerous propensities of a thoughtless period of life, and to impress upon them the worthlessness of all attainments which are not made subservient to the high destinies of an immortal being. Attached to the interests of the institution with which he was connected, he scorned to sacrifice its useful-

* He was always very fond of the remarkable saying of Boerhaave's, "that he considered the poor his *best patients*, because God was their pay-master."

ness to his own emolument; and abhorred the sordid policy which has, in later times, disgraced so many of our medical schools, and degraded their highest dignities to mere offices of profit.

As a public officer entrusted by the authorities of his country to secure his fellow-citizens from the importation of pestilence, we find him discharging the duties of his hazardous appointment with a fidelity which odium and reproach could not turn aside; and with a boldness which even constant exposure to danger could not intimidate or alarm. He was in this station, as in every other which he accepted, strictly *conscientious*. He did not, by a convenient casuistry, accept the superintendence of an establishment which, in his conscience, he believed to be founded upon a fallacious opinion, and grievously oppressive to the commercial prosperity of his country. His professional morality was regulated by a higher standard. He believed fully in the principle which gave rise to the quarantine institution, and in the necessity for the rigid enforcement of its laws. And, accordingly, during the term of his appointment, our city was a stranger to the horrors of pestilence.

As President of the College of Physicians, Dr. Bard continued for many years to watch over the destinies of medical science, with a dignity which commanded the respect of all the officers of that institution; and with an impartiality which, preserving itself indifferent in the petty conflicts that occasionally arise in every similar body of men, permitted him to do no injustice to contending parties, but kept him faithful to the true interests of his trust. In viewing the character of Dr. Bard as the president of the college of physicians, I might advert to the unhappy disturbances which lately occurred in that institution; and which, much against his inclination, involved him after he had lived upwards of seventy years a stranger to discord. But the developement which, as a faithful biographer, it would be incumbent upon me to make, however much it might redound to the honour of Dr. Bard, would revive recollections of no agreeable nature in the bosoms of his friends, and might again excite the rancour and the madness of his enemies. Peace be to his ashes!—I have no wish to awaken the sacrilegious feeling of hatred to his memory.

Dr. Bard, as an AUTHOR, deserves and holds no humble station. He was not indeed one of those mighty geniuses who have occasionally adorned the profession of medicine; and who, spurning the ordinary course pursued by men of humbler powers, strike out for themselves a new road to the temple of fame. He invented no brilliant theories;—and devised no new system. Nor did he possess the ambition of being distinguished as an author. He did not write much: but what he did write was always useful, and always exhibited his admirable talent of imparting to every subject an interesting aspect and a practical cast. His style was remarkably simple and chaste. Unadorned, it was, however, pleasing. Not vigorous or eloquent, it was clear and concise; well suiting his sober method of thinking and reasoning, and judiciously adapted to the subjects he discussed. Yet with all its humbleness and all its simplicity, there ran through it a vein of classical purity and taste which belongs to no indifferent scholar. In short, it was characterized by a strain of “truth and soberness” singularly felicitous in captivating the attention.

His first literary production, an Inaugural Essay on the powers of opium,* would not have been unworthy of his pen in the brightest period of his fame. Essays of this kind are usually of a very puerile character, having no higher aim than to comply with the requisitions of an academic statute, and seldom venturing upon any subject which is not elementary or trite. But Dr. Bard chose a subject which, while it was difficult in itself, was rendered still more intricate by the controversies to which it had given rise. At the time he wrote this experimental essay, the powers of opium, the mode of its operation, and its various effects upon the body, were but imperfectly understood; and were matter of much difference of opinion among the profession in Edinburgh. He, therefore, resolved to investigate the subject experimentally. After many well conceived and well conducted experiments upon himself, his fellow-students, and the patients of the Royal Infirmary, he came to the conclusions: 1. That opium produces its effects by acting primarily upon the brain and nerves. 2. That it lessens the frequency of the pulse, but increases its fulness.

* *Tentamen medicum inaugurale de viribus opii.* 1765.

3. That its primary effect upon the system is to induce hilarity. 4. That it diminishes animal heat. 5. That it diminishes all the secretions except the perspiration, which it increases. 6. That it constipates the bowels. 7. That it lessens the urinary discharge. 8. That it renders respiration slower. 9. That it produces a sense of fulness and stricture about the head and chest. 10. That it assuages pain, resolves spasm, and recruits the body exhausted by fatigue, &c. All these positions are supported with much ingenuity, and explained with a readiness which shews him to have been master of his subject, and familiar with every thing which had been written upon it. And while this dissertation is a memorial of his respectable attainments in his profession; it is a creditable specimen of his literary and classical acquirements.

In the year 1769 Dr. Bard delivered to the first medical graduates of King's (now Columbia) College, a discourse upon the duties of a physician, in which he endeavoured to impress upon the public the necessity and importance of an hospital in the city of New-York. So great was the effect produced by this memorable discourse, that in the very day on which it was delivered the sum of £.800 sterling was subscribed towards the erection of an edifice for that purpose.* When nearly completed it was destroyed by fire in the year 1775, and in consequence of the Revolution was not rebuilt until 1791. This second building remains to this day—a magnificent monument, "*aere perennius*," to the memory of Dr. Bard. This discourse was immediately published at the request of Sir Henry Moore, at that time provincial governor of New-York.

He shortly after this, in 1771, published "An enquiry into the nature, causes, and cure, of the Angina Suffocativa, or sore-throat distemper," as it was then vulgarly called. This disease, it seems, had but lately appeared, and had committed great ravages among the children of the inhabitants. From the description Dr.

* The corporation of the city soon added £3000 to the first subscription, and the legislature resolved to make the liberal appropriation of £800 per annum for twenty years. Dr. John Fothergill and Sir William Duncan of London deserve to be mentioned for their benevolent exertions among the inhabitants of that city in behalf of this laudable charity.

Bard has given of it, there is no doubt it was a *Croup* of a highly violent and malignant character. In this valuable treatise may be found *blood-letting* suggested as a remedy, although claimed in later times as a discovery; and *calomel*, for the honour of introducing which so many have contended, is there recommended as possessing advantages "beyond any other medicine." The style of this little essay is plain and concise, exactly that in which a popular treatise should be written.

Dr. Bard's favourite branch was midwifery. And perhaps no physician in this country has ever enjoyed a larger share of practice in this department, or acquired a higher reputation as an accoucheur than he. After retiring into the country from the cares and fatigues of professional life, one of the first plans of usefulness contemplated by Dr. Bard, was the publication of a treatise upon this subject. His residence in the country, and the celebrity he had acquired as an obstetrician, afforded him frequent opportunities of witnessing the ignorance of midwives and country-practitioners upon this important branch; and determined him to issue a treatise which, while it should contain a set of plain and practical directions for the management of natural labours, should possess the advantages of cheapness and conciseness, and be divested of technicalities and professional idioms. Accordingly in the year 1808 he published "A Compendium of the theory and practice of Midwifery" in a comely duodecimo form, intended chiefly for the use of midwives and young practitioners. Unassuming as were the claims, and humble as was the object of this little compendium, it was executed in a manner which would not have dishonoured a work of much loftier pretensions. Arranging and condensing all the valuable precepts delivered in the volumes of the standard writers on obstetrics, it contains the results of the experience which he had derived from a long and extensive familiarity with the practice of the art. Accordingly it met with immediate and universal approbation, and has ever since its appearance, received the suffrages of the profession as a standard work. It was more particularly Dr. Bard's design in this little volume to treat of natural labour; and to insist upon the sufficiency of the efforts of nature in these cases. He wished to repress the growing

fondness which the popular works of Smellie and Baudelocque had introduced into this country for the use of instruments; and to convince practitioners, that they are seldom, very seldom, required. Notwithstanding his modest renunciation of all claim to originality, I verily believe that the powers of nature in the accomplishment of parturition, have never been so forcibly and clearly exhibited by any other author. The work went through three large editions in its duodecimo form; and was twice published greatly enlarged and improved in octavo. At the time of his death he was preparing for the press a *sixth* edition*. And although it has not received the finishing touches of his experienced hand, it will, no doubt, add increased excellence and celebrity to the work; and live to perpetuate the name and the reputation of its author.

It was Dr. Bard's ruling desire to be useful. Accordingly in the year 1811 he published "A Guide for Young Shepherds," at a time when the commercial restrictions which a short-sighted policy had imposed, were turning the attention and the enterprise of our citizens to the establishment of domestic manufactures. Among the internal improvements, the melioration of our breed of sheep by the introduction of the merino race, was a favourite project. But it was soon found that, while the sheep imported from abroad were very delicate and extremely liable to disease, they very often communicated to our native flocks distempers to which they had not before been subject; and which, while they rendered such stock exceedingly precarious, repressed the enterprise of commercial adventurers. It was all-important, then, that something should be done to prevent the defeat of the project by the frequent disappointments incident to speculations upon sheep. At this juncture Dr. Bard, after having directed his attention to, and acquired considerable experience in, the diseases of this useful animal, issued the little treatise of which I am now to speak. It would not become me to pronounce upon the merits of this book as a work upon the diseases of sheep, as I have no practical acquaintance with the subject to which it re-

* This will shortly be published under the direction of his grand son, Dr. F. U. Johnston of New-York.

lates, and am not sufficiently conversant with rural concerns to justify me in forming even an opinion of its value. I can only say, that I have known intelligent and competent persons who have turned their attention to the management and improvement of sheep, declare it to be the best practical treatise extant upon that subject, the masterly performance of Chancellor Livingston not excepted. In the *Shepherd's Guide* Dr. Bard treats of the characters and qualities of the merino-race, and of their great superiority over our native sheep. He considers the most approved mode for the maintenance and support of sheep in general; remarking upon the quality and quantity of their food, their summer and winter management, the proper construction of barns for the storing of their provender and of hovels for their shelter, and a variety of other interesting circumstances connected with the successful improvement of the domestic breed, and the acclimation of the merino race. He discusses fully the subject of breeding; and enters minutely into the management and rearing of lambs. The diseases of sheep occupy a considerable portion of the treatise. In this part of the book the author gives practical instructions for the discovery and treatment of all the most troublesome and frequent distempers to which the animal is liable; and dwells particularly upon the singular malady known by the names of *claveau* and *variola ovina*. In the treatment of this malady he considers the subject of inoculation, which the cultivators of sheep in Europe have recommended as a preservative from the fatal forms of this distemper. But he found that the disorder could seldom be communicated in this way; and was not thus mitigated in violence. I know not whether Dr. Bard is entitled to the merit of originality in suggesting and trying the prophylactic virtue of vaccination in this plague. He, however, vaccinated several sheep, encouraged by the analogy which exists between this distemper and the small-pox in the human race; and recommends the practice to shepherds. This treatise preserves the unity of style which Dr. Bard exhibited in all his writings. It is plain, sententious, and well adapted to the practical character of the work.

It is to be regretted that the annual addresses which, as presi-

dent of the college, he was in the habit of delivering to the medical graduates, have not been published. The publication of one of them, "a discourse on medical education," which was delivered at the commencement in 1819, serves only to make us regret the suppression of the rest. They would form a very comely volume, instructive to the profession, and interesting to the general reader. They were remarked for the strong good sense with which they abounded; and for the tenderness and pathos of the sentiments they contained. They were written with the characteristic chasteness and neatness of Dr. Bard's style; and delivered with the simplicity and ingenuousness peculiar to his manners.

The last production of his pen was a paper containing "Remarks on the Constitution, Government, Discipline, and Expenses of Medical Schools, &c." This document was drawn out in obedience to a requisition of the Regents of the University. In it he enters into an examination, minute in proportion to the importance of the subject, of all the different branches of medicine. He inquires into the relative value and importance of each in a course of medical instruction; and into the manner in which they should be taught. From an elaborate comparison of the expenses of the medical schools of Edinburgh, Philadelphia, Boston, and Baltimore, he endeavours to establish a standard for the regulation of the fees and expenses of the college of New-York. He makes some excellent remarks upon the preliminary education proper for students of medicine, the period of study which should be required, the requisitions for graduation, and the mode of conducting examinations; and finally sketches an admirable plan for the organization of a medical school. Peculiar circumstances at that time interfering, the improvements suggested by Dr. Bard have not all been carried into effect. But should that honourable body ever deem it expedient to reorganize the medical department of the University, there is no doubt that the plan recommended by him will be their model.

Dr. Bard's views upon the subject of medical education, were not influenced by the contracted policy which would restrict the studies of a physician to that kind of knowledge which may en-

able him to practise his profession as an art, but which can never qualify him to maintain its literary character. Nor yet did he approve of introducing into courses of medical instruction, those collateral sciences whose connexion with medicine is so remote that they can have no tendency to improve the practical departments of the healing art. While he desired that the studies and acquirements of a physician should be extensive and general, he knew that an undue attention to collateral branches is one of the most effectual hindrances to the perfection of medicine. He, therefore, wisely concluded, that it is sufficient for a physician to possess that general acquaintance with them which will enable him to understand the conversation of the learned in these subsidiary departments; and to maintain a reputation as a scientific man.

On the subject of the preliminary education to be required of students, Dr. Bard regretted that the present state of things rendered it inexpedient to insist upon any degree of classical attainments. He was well aware of the great value of these studies to a physician; and wished that it were possible to withhold the doctorate from those who were not qualified for the inferior honour of a bacchalaureate in the arts. It is remarkable that colleges erected for the purpose of teaching a learned profession, have so universally fixed the professional standard at so low a rate. Almost all nations have agreed in assigning this exalted character to the medical profession; and in expecting from all who practise the healing art, a degree of erudition not required of men of other occupations. Hence, among the ancients the priests were almost the only practitioners of physic, because always the most learned class of men. Thus we find, that the Chaldeans of Babylon, the Hierophantes of Egypt, the Curetes and Corybantes of Crete, the Persian Magi, and the Gymnosophists of India, were in their respective countries the most distinguished cultivators of medicine. Even in modern days this association of the medical character with the sacerdotal office has existed. It was not till the middle of the twelfth century, (1163) when the council of Tours pronounced the famous edict which forbade the clergy the exercise of the healing art, because the Church abhorred the shedding of

blood, that the practice of medicine was followed extensively by the laity. Among all the semi-barbarous nations of the present day the priests are still the only physicians. Even in Persia, the Lamas alone practise medicine. And it was not until the immense increase of population rendered the care of the sick incompatible with the burdensome parochial duties of clerical life, that the clergy of New-England ceased to practise physic. It is evident, then, that the profession of medicine is a *learned* one. How comes it that its highest honours and its greatest privileges are conferred upon men who are destitute of the very elements of a learned education? Dr. Bard saw and lamented the error, but saw no way to remedy the evil. Perhaps this can never be done but by a national university.

In his hints for the reorganization of the college, he very properly recommended that the trustees should be a body of men separate from, and independent of, the professors. He saw that every medical institution in which the professors are vested with the powers of trustees, must continually be harassed by jealousies and contentions: and that if the respectability of the college is to be preserved, and a diploma is to be no longer a mere certificate of attendance upon lectures, the board of trustees must be composed entirely of persons who have no pecuniary interest in the concerns of the college.

As far as Dr. Bard has considered the subject of medical education, the measures he recommends in his report to the Regents for the improvement and reformation of the present system, are exceedingly judicious, and well calculated to effect the purpose for which they are designed. Yet, it may not be inconsistent with the office of a biographer to point out two capital defects in his plan. The first is, that no provision is made for the *admission* of young men to the study of the profession. Ours is the only one upon which a person may voluntarily enter, without being subject to control. Before being admitted as a candidate for holy orders, a young man is required to produce testimonials of his possessing certain qualifications, without which he is not recognized by the authorities of the Church as a regular student of divinity. So it is indeed, in some kind, in almost all occupa-

tions and trades. But in medicine, it would seem, we receive the refuse of them all, without applying any test whereby a judgment may be formed as to the *fitness* of the individuals to commence the study. The Mexicans, we are told, determine the occupations to which they send their children, by observing the selection which they make from the tools of different trades when the operation of the judgment is suspended by intoxication. Judging from the stupidity and unfitness of many of the profession who have neither knowledge nor the capacity for acquiring it, we are almost induced to believe that their unfortunate choice must have been made under a similar infatuation. Juan Huartes, a Spanish physician, in a book entitled "*Examen de Ingenios*,"* proposes the appointment of a board of public examiners, whose duty it should be to ascertain the genius of every lad, and to assign to him some suitable occupation to which it should be obligatory upon his parents to place him. This may be carrying the matter rather far; but the advantages of some such inquisition would certainly be very great. An anecdote of Clavius, the celebrated mathematician,† illustrates the propriety of scrutinizing the peculiar talents of boys. He was sent to a college of Jesuits; and at his examination for reception was about being dismissed as a hopeless dunce, when being accidentally asked some question in geometry, he was discovered to possess that turn for the mathematics which afterwards distinguished him as one of the first astronomers of his day. Indeed the figure which this order have made in the learned world is, no doubt, ascribable, in a great degree, to their sagacity in discovering the talents and directing the studies of their pupils. It is because sufficient care is not taken to ascertain the peculiar propensities and geniuses of young men, "that we see so many men in situations for which they are not qualified, who would have been respectable or useful in the professions or trades for which they were born. They form the

* This book, of course, I have never read. Some account of it may be seen in the Spectator, No. 307.

† Christopher Clavius, a German, who was sent for by Pope Gregory to assist in reforming the calendar; and afterwards engaged ably in its defence against the attacks of Scaliger and others.

same disease in society, which is known among physicians by the name of *error loci*. They are like red blood in serous vessels, bile in the stomach, and aliment in the wind-pipe."* To devise some plan whereby some control may be exercised over the admission of young men as regular students of medicine, and the profession preserved from being overrun by the blockheads whom the blindness of their parents would thrust into it, is yet a *desideratum*.

Another important defect which Dr. Bard has overlooked, is the want of some fixed and determinate course of medical study. It would be well to select for the student the books which he should read; and to point out to him the order in which they should be studied. Nor is this sufficient: the very manner in which they should be read ought to be made known to him. Lord Bacon says that there are some books which should only be tasted; others that we ought to swallow down; and some choice ones which we should chew and digest: meaning thereby, that some books should be only partially read; others should be read, but without much care; and a few should be diligently and attentively studied. Thus to direct the industry of the student by properly disposing of his time, selecting for him suitable books, and pointing out to him their relative value, would be a grand improvement in the system of education.

These ideas may have occurred to Dr. Bard in drawing up his plan for medical study. But his object seems to have been rather to reform abuses, than to introduce innovations in the present system. It was, doubtless, on this account that we find no reference to his favourite idea of introducing the writings of the ancients more generally into a course of medical reading for students. He always held in high estimation the works of Hippocrates; and desired particularly to revive a taste for classical medicine in general. It is to be regretted that gentlemen of the profession so generally regard the writings of the father of medicine and the other ancient masters of the healing art, not worth their study. So deeply convinced were the faculty of the *Ecole de Médecine* at Paris of the value of Hippocrates, that they, some years since, instituted a *Hippocratic Professorship*, for the purpose of awakening the

* Rush's Lectures, p. 358.

attention of physicians to the ancient medical writers, and especially to preserve from oblivion the doctrines and practice of the Coan sage. This professor was to be supplied with the means of travelling, with the volumes of Hippocrates in his hand, over the countries which the father of physic had visited to observe diseases; and was to lecture exclusively upon the works of Hippocrates. This scheme may have been injudicious, but it was magnificent. Perhaps the most effectual plan would be the institution of a professorship of *Medical Literature* in general. The duties of this appointment might embrace instructions on medical history, including bibliography or an account of the different systems and books; medical jurisprudence, medical ethicks, medical logic, and perhaps too expositions of and recitations from the Greek and Latin medical classics. But I fear that this critical examination of Dr. Bard's views of education may be considered by the reader as inapposite, or at least as too extended for a biographical memoir. I must plead as an apology for my prolixity, the impossibility of doing justice to the character in a narrower compass.

I have thus noticed all the writings of Dr. Bard with which I am acquainted. Several fugitive essays by him are preserved in the American Medical and Philosophical Register; and other periodical journals are, I believe, enriched by his communications. The Transactions of the College of Physicians of Philadelphia contain several documents by him on the subject of Yellow Fever. In these Dr. Bard, in a tone of firmness and decision worthy of the serious convictions he entertained of the foreign origin and contagious character of the disease, avows himself one of the persecuted minority who have advocated these doctrines. I say a *persecuted* minority: for it cannot be denied, that the leaders of the opposite party have prosecuted the controversy with an acrimony sufficient to deter a timid man from declaring his sentiments, and a peaceable one from defending them.* Dr. Bard always

* I speak here in reference to the controversy as it has been conducted in New-York only. Indeed here it has become a *personal* question. Will you side with certain men, or will you join their enemies? The violence on this subject, and the aspect which is given to the controversy, is, no doubt, intentionally excited by designing men. They have taken this road to *importance*, because they cannot succeed in becoming the leaders of a party by more honourable means.

regretted the injurious agency of certain periodical journals in propagating doctrines of an opposite tendency; and deprecated the system of denunciation pursued by works from which a spirit more worthy of the votaries of science, ought to have been expected.

Having considered Dr. Bard as a professional man, as a public character, and as an author, it remains to contemplate him in the interesting scenes of private and domestic life.

Accustomed from early life to the best and most polished society, Dr. Bard always exhibited in his deportment and manners a perfect model of the accomplished gentleman. In the several relations of a son, a husband, a father, and a friend, he was a pattern of filial affection, of conjugal fidelity, of parental tenderness, and of unwavering and ardent attachment. The moral virtues shone conspicuously in his character. His integrity and uprightness were proverbial. To say that a man was "as honest as Dr. Bard," was in his neighbourhood the very highest recommendation for stern and unbending integrity. He was charitable and liberal, almost to his own ruin. Indeed for several of the last years of his life, he appropriated almost the whole of his annual revenue to a benevolent purpose, reserving for himself scarcely a comfortable competency.

But this was not all his character. Dr. Bard was a CHRISTIAN. Nor was he a Christian in the vague sense in which this honourable name is applied by the world. He was not a mere speculative believer in the truth of Revelation; he was not a mere respectful attendant upon the services and ordinances of the sanctuary; he did not view religion as a mere system of ethicks which might or might not be received, or which at most exacts nothing more than a decent conformity to the requirements of morality. His piety was of a much more sterling stamp; exhibiting in the affections of the heart, in the tempers of the mind, and in the conduct of the life, the sanctifying and practical power of Christian principle.

I know that it is fashionable for biographers to cant about the piety of those whose characters they portray; and to represent

as the brightest ornaments of the Christian name, men whose lives have not been remarked for more than common morality, and actions to which a sense of honour, instinctive and unsanctified, may have prompted. But the fact is, education, habit, interest, and many other circumstances, may develop the virtues of justice, and integrity, and compassion, and generosity in the human heart, and may even enkindle some feelings of superstitious reverence for religion; and yet a man exhibiting all these traits of character, may not in a single action of his life be actuated by a principle of loyalty and obedience to God, and may be an utter stranger to the radical principles of equity and benevolence. Nay, a man may maintain an exalted character for strict justice, high honour, generous sensibility, and for every manly and effulgent virtue; and yet be as destitute of all claim to the title of a *Christian*, as the vilest profligate whose life presents one disgusting mass of moral deformity unredeemed and unrelieved by a single amiable feature. I would not decry morality—it is useful, it is amiable, it is necessary to the well-being and the good order of society. But *alone*, and unconnected with holier principles, it is no more acceptable and meritorious in the sight of the Supreme Ruler and Judge of the world, than the constrained obedience which a discontented subject may yield to the laws of his country, or the kindly offices which rebels may mutually interchange, can be to the authorities to whom they owe a dutiful and cheerful loyalty. Religion is a divine principle which enlightens the understanding to the comprehension of truths that unassisted reason could never have discovered;—a principle which rectifies the waywardness of the will, and brings it into subjection to the law of God;—a principle which reclaims and refines the corrupt propensities and passions of our nature, purifying the very thoughts and affections of the heart;—a principle, in short, which renews and sanctifies the whole man, and preparing him for the acceptable service of his Maker here, fits him for the blissful enjoyment of his presence hereafter. *Such* was the religion of Dr. Bard, and such only can entitle him to a character for piety.

Nor did his views of the nature and importance of religion, suffer him to rest satisfied with the possession of personal piety. No!

He viewed religion as a concern in which all mankind are interested, deeply and eternally interested. Accordingly we find him exerting all his influence to disseminate the Holy Scriptures, and to extend the benefits of the services of religion in his neighbourhood. We find him the ready patron of every scheme which Christian benevolence might devise for the promotion of religious knowledge, and of human happiness; and evincing that his labours of love did not proceed from ostentation and parade, by private exertions which could procure him no applause from men. Bible Societies, Missionary efforts, Sunday-schools, and the humbler attempts to diffuse religious instruction by Tracts, all found in Dr. Bard a prompt and zealous friend.

He was one of those very few physicians who consider it a duty to admonish and advise their patients in their spiritual affairs. It was his constant practice to procure, or to administer religious instruction to the ignorant, and spiritual consolation to the distressed. And however indiscreet and officious communications of this kind may be considered by some, he has left upon record his testimony to their usefulness, and to the general goodwill with which they are received. In not one of the many manuscripts (in my possession) of his annual addresses to the graduates in medicine does he omit to recommend this practice; and to enforce it by the assurance that during thirty years of professional life he had made it a uniform duty, and that he had very seldom regretted his conduct, having found such communications to be generally acceptable, and never productive of injury to the sick. It is very much to be regretted that the example of this good physician is not more frequently imitated; and that medical men are so apt to disregard the eternal concerns of their patients, and to imagine that it is even necessary to divert their thoughts, as much as possible, from death and eternity. Such conduct is a criminal neglect of a solemn duty; and betrays an insensibility as cruel as it is dangerous to the best interests of those committed to their care. It was too Dr. Bard's practice to call the early attention of his patients to this important subject. Religious admonition, he properly thought, should not be deferred until all hope of recovery is gone. This is not the best chosen

period for religious instruction, or the one most favourable to its due effect upon the mind. It is not in the last moments of life, when the body is racked with pain, and the mind agitated and alarmed by the apprehensions of death; when a deadly stupor clouds the faculties, or the imagination flits in wild delirium from object to object and from thought to thought, that the mind can be brought to prepare itself for the awful transition which it is to undergo. Sickness is a season of reflection with most men, and naturally induces a docility of temper highly favourable to the reception of wholesome admonition. It is now that religious instruction and advice are most productive of effect. If delayed till the last hours of life, they may serve indeed to awaken the alarms of the sick man, and to plunge him in despair; but they can seldom benefit his soul.

The conduct of Dr. Bard in this particular must commend itself to the approbation of every rational and feeling man; and entitle him to be placed with those worthies who have united to exalted talent, extensive erudition, and distinguished rank, the graces and virtues of the Christian character; and whose lives have practically refuted the scandalous proverb, "*Medicus non Christianus*."*

I know not what effect this feeble and imperfect representation of the life and character of Dr. Bard, may produce upon the reader: I should hope that the outlines of such a character, however rudely sketched, would fill him with admiration. But, for my own part, I must confess, that the very name of Bard has in it for me something of that magic power which one feels to exist in the names of Boerhaave, and Sydenham, and Gregory, and Rush; and which, while it excites an enthusiasm that no other earthly contemplation can produce, annihilates the ambitious aspirations of the soul in overwhelming admiration of these illustrious men.

* Haller, Boerhaave, Sydenham, Stahl, Hoffman, Harvey, Willis, Mead, Zimmerman, Fothergill, Percival, Heberden, Gregory, Rush, Ramsay, &c.

ART. II. *Two cases of Extraordinary Tumours formed on the external surface of the Uterus.* Communicated in a letter to Henry W. Ducachet, M. D. by JOHN W. FRANCIS, M. D. Professor of Obstetrics and the Diseases of Women and Children in the University of the State of New-York, &c.

New-York, August 1, 1821.

DEAR SIR,

I NOW send the account, some time since promised you, of the two cases of Extraordinary Tumours formed on the external surface of the Uterus. I believe you will find them sufficiently interesting for publication. I have searched in vain for any recorded evidence of instances of similar tumours of the uterus, and upon inquiry I find they are novel to my friends in the profession. Baillie and Clarke, as well as other writers, have described at some length internal tumours of this organ, and have noticed the occasional liability of its external surface to small tubercular excrescences. I am of opinion that these external tumours are of more frequent occurrence, and often of a more formidable character, than is generally supposed.

The two cases which I now communicate came within my observation during the spring of 1821.

I remain, Dear Sir, yours sincerely,

JOHN W. FRANCIS,

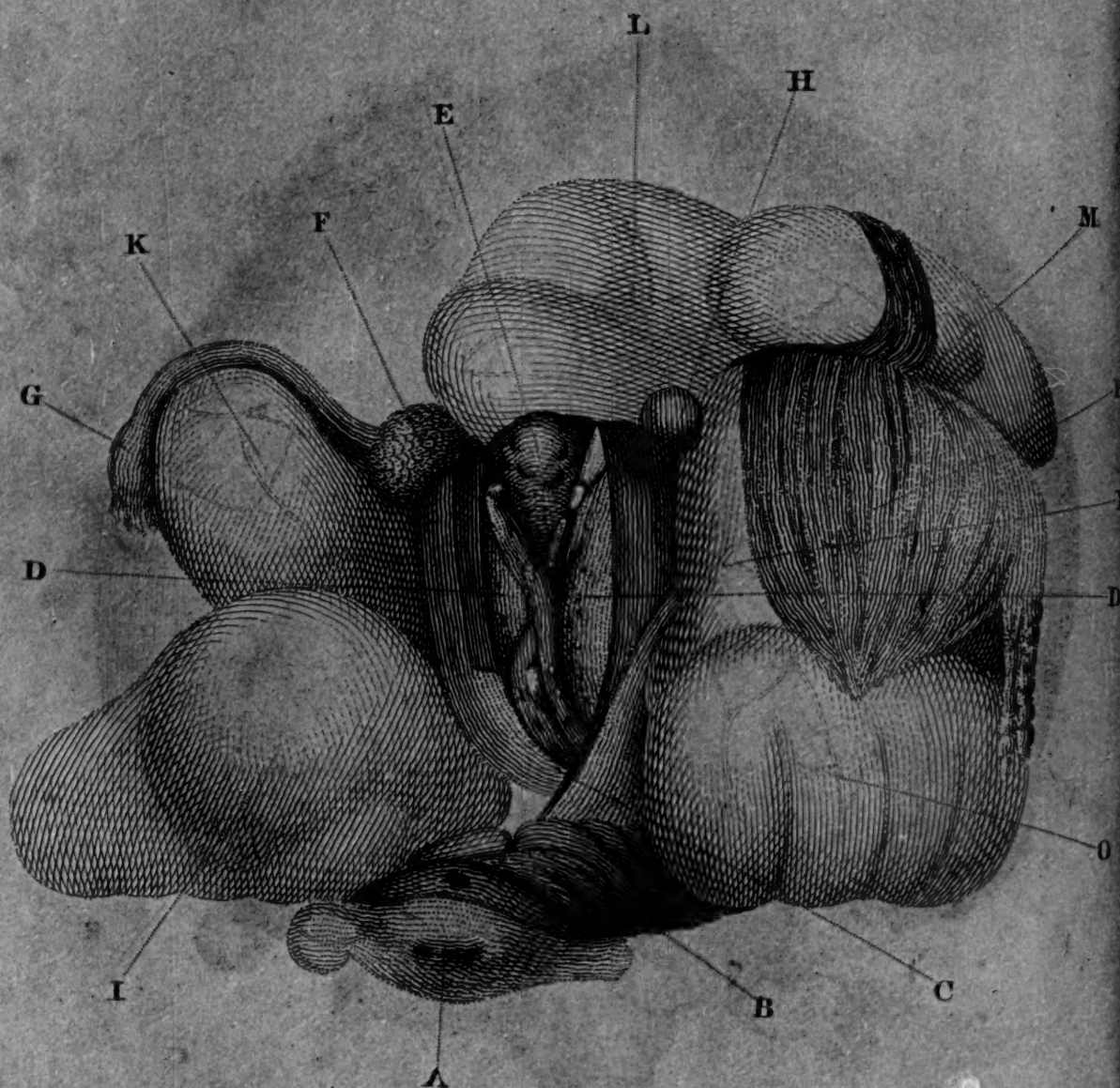
CASE I.

The first case occurred in a patient aged thirty-two years, of a delicate form of body and of temperate habits of living. She had been married six years, but never proved pregnant. She suffered from her complaint about five years, when she died suddenly, and rather unexpectedly. During the whole of her illness, which was seldom so painful or annoying as to prevent an attendance on the ordinary duties of her house, she regularly menstruated, though the discharge was smaller in quantity and thinner than natural. The greatest inconvenience she laboured under, was the weight of the tumour, and its pressure upon the abdominal

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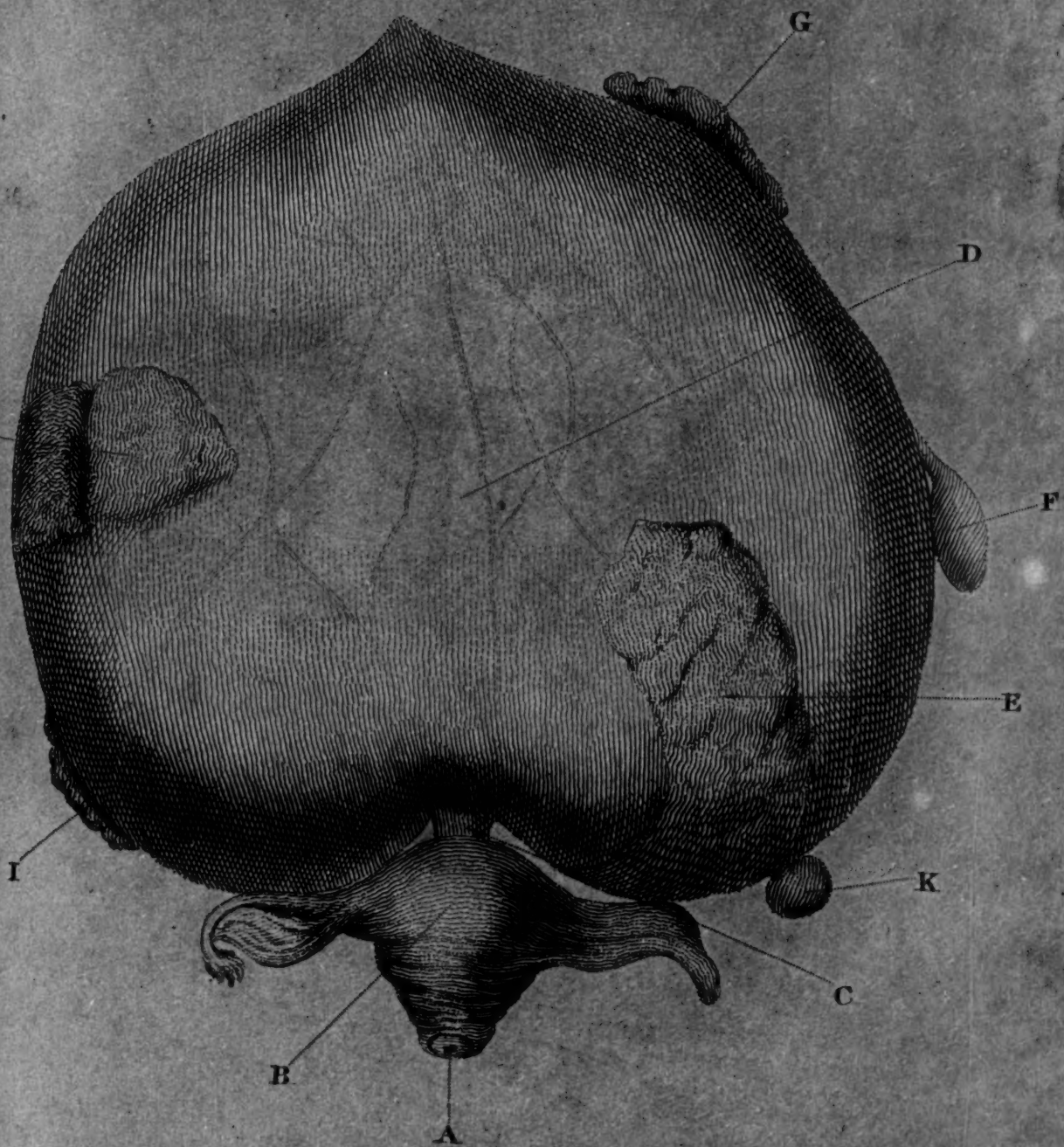
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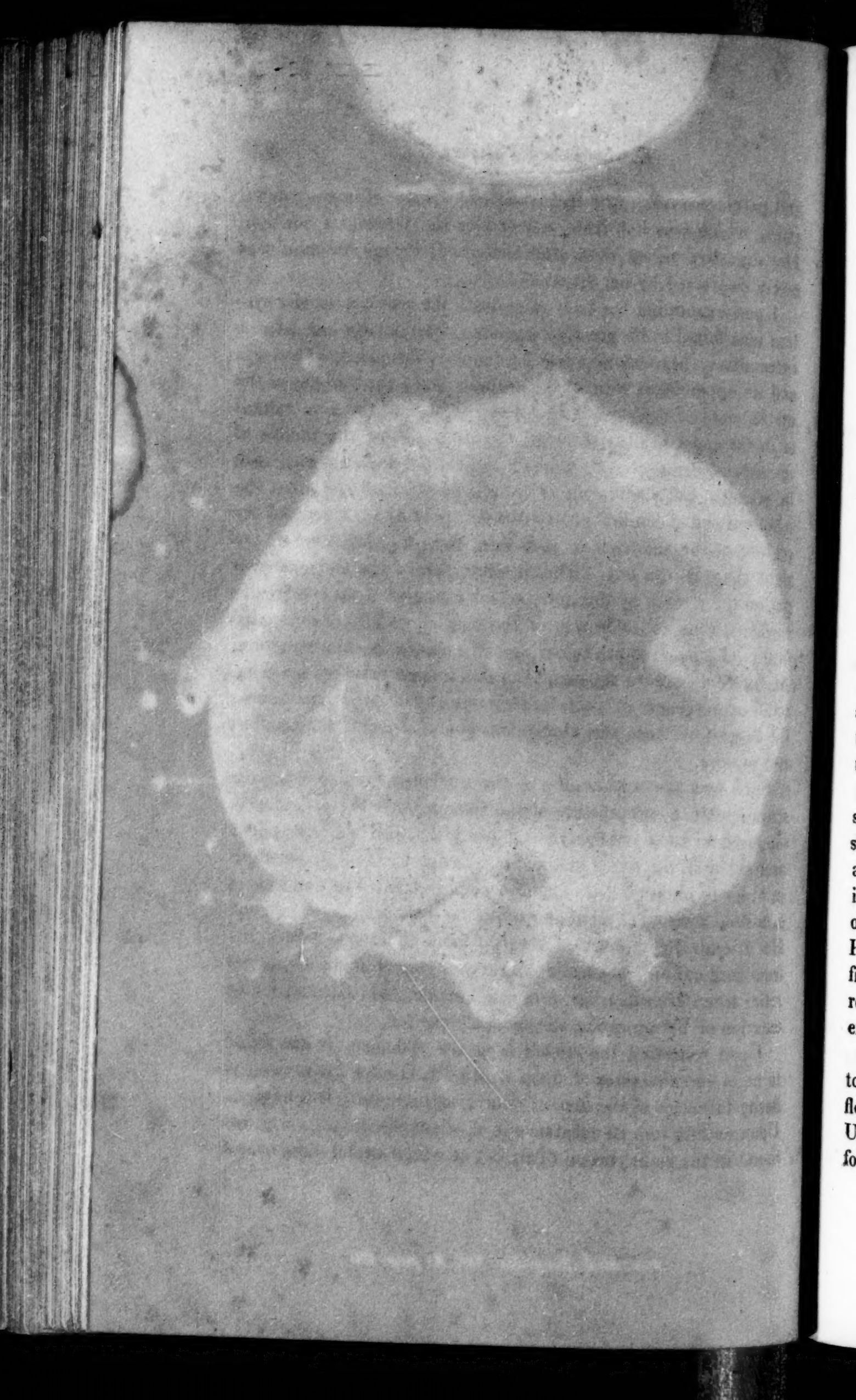


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and pelvic viscera. She frequently had a sense of bearing down pain, which was but little relieved by the horizontal position. Her digestive organs were often disturbed; though her mind was rarely depressed by her situation.

Upon examining her body after death the whole muscular system was found to be greatly emaciated, particularly her inferior extremities; her abdomen was prodigiously distended; the uterus and its appendages were of the ordinary size: the parietes of the uterus were of their usual thickness; and the os tinæ as natural as in the most healthy state of the parts. From the fundus of the uterus, connected by a small pedicle not more than an inch in breadth, grew a tumour of a bulk so extraordinary, that the whole of the abdominal cavity was occupied by it: it pressed the regions of the abdomen on each side, though rather more on the right than on the left. The anterior part of the abdomen was put on the stretch by this mass, which extended to the scrobiculus cordis. This tubercle was of the species usually denominated fleshy, of a pale reddish colour, and of a fibrous vascular structure. At different parts of the surface of this tumour grew out six other small excrescences of a similar structure and tattered appearance: the largest of these was almost two pounds weight, the smallest two ounces.

Such was the appearance of the abdomen, arising from the shape, texture and pressure of the tumour, that the patient was supposed for some time previous to her death to be labouring under ascites: and she twice underwent the operation of paracentesis in order to obtain relief: on these occasions three or four ounces of a fluid somewhat albuminous was the only discharge obtained. Her friends being satisfied that no relief could be afforded her from surgical aid, she had recourse to a variety of means for her relief when her sufferings were at all severe; and after an undue exertion of her strength, she suddenly expired.

Upon removing the tumour from the abdomen, it was found to be of the character of those which Dr. Hunter has noticed as fleshy tubercles of the uterus. Its fibrous structure was evident. Upon cutting into its substance in different directions, a sac was found in the right portion of it, out of which issued three quarts

of a purulent and most offensive matter. - This condition of the tumour may account for the fluctuation which seemed to be felt by striking the abdomen; a peculiarity which is not to be observed in mere fleshy tubercles of the uterus. According to evidence taken at the time of examination, the tumour and its excrescences weighed rather more than one hundred pounds. The circumference of the abdomen before the removal of the tumour, notwithstanding her marasmus, measured four feet eight inches and a half.

The above case occurred in the practice of Professor Mott, of the University of New-York.

CASE II.

The other instance of fleshy tubercle to which I have alluded, happened in this city in April last. The patient was aged about forty-four years. She had enjoyed good health until within the last two years of her life: at this time she was supposed to be pregnant, and had many of the symptoms which attend that state; as the cessation of the menses, and occasional irritation of the stomach and bowels. Toward the end of what was supposed to be her full time, she was accidentally exposed to cold, to which she attributed the sickness which took place shortly after. At this time she believed that the slight symptoms which she suffered were those of approaching labour: but these symptoms soon disappeared; and for more than twelve months subsequently her principal complaints were an acrid discharge from the uterus, which was irregular, both as to the time of its appearance and its quantity, a sense of great fulness and oppression at and around the region of the symphysis pubis, and slight irritation of the bowels and urinary organs. She took little more than mild aperient medicines to mitigate her symptoms, and died apoplectic.

Upon opening the body, the uterus was found to be enlarged to the size it usually attains at the sixth month of pregnancy; its parietes were thickened to the extent of an inch and a quarter, and the substance itself was in many parts in an ossified state. At its fundus, internally, there was attached by a short pedicle a round bony tumour about the size of a small orange. The right

fallopian tube, at a short distance from the part which opens into the uterus had, also, a round bony tumour of a firm and granulated appearance; the ovarium on the same side was larger than ordinary. There were other appearances still more extraordinary. Near the cervix of the uterus there grew out by small attachments six large tubercles of an oblong shape, which so branched out that the whole inferior portion of the abdomen, nearly as high up as the umbilicus, was occupied by them. They were of a vascular texture, of a pale reddish colour, of a very firm and resisting nature, and, in many places, had evident marks of ossification. Two other smaller excrescences were discovered, one growing from one of the tubercles, and the other from the fundus of the uterus, of the size of a walnut. The tumours, and the diseased uterus, were found to weigh thirty-two pounds four ounces.

I am indebted to Mr. L. Proudfoot for an opportunity of examining this remarkable specimen of disease.

The annexed engravings will perhaps convey a better idea of the situation and extent of these tumours than any description.

PLATE 1.

- A. The Os Tincæ.
- B. The Uterus and its appendages.
- C. The Neck of the tumour.
- D. The Tumour.
- E. F. G. H. I. K. L. Smaller Tumours or Excrescences growing from the great Tumour.

PLATE 2.

- A. The Os Tincæ.
- B. The Bladder contracted.
- C. The Uterus.
- D. D. The Parietes of the Uterus, thickened and in several parts ossified.
- E. The Bony Tumour, growing internally from its fundus.
- F. The Tumour in the fallopian tube.
- G. The enlarged ovarium.
- H. A small Tumour from the fundus of the Uterus.
- I. K. L. M. N. O. Six large Tumours attached near the Cervix Uteri.
- P. A portion of the Peritoneum.

ART. III. *A Case of Hydrocele, in which sabulous matter was discharged by an operation.* By HENRY W. DUCACHET, M.D. of New-York.

It is impossible to calculate the value of a fact in medicine. For, however trifling and unimportant it may appear, it may lead to the discovery of important principles in the science, and perhaps even to the developement of great improvements in practice. But without any anticipation that the simple fact which I am about to communicate, will lead to any such results, I consider it proper to place it upon record for the sake of its singularity.

In April last a boy about thirteen years of age was brought to my house, for the purpose of procuring advice on the subject of a hydrocele with which he was affected. It seems that it had come on without any known cause about two months before, and that the accumulation had been very rapid. I immediately tapped it in the usual way, and let out about four ounces of a transparent, straw-coloured fluid. In the bottom of the cup in which the water had been received from the canula, I discovered several small granular bodies, which, upon examination, proved to be sabulous. I requested the mother, who accompanied him, to send him to my house as soon as she should perceive any return of the disease. But I have not seen him since.

It is related of the celebrated John Wesley, that in riding about in his truly apostolical peregrinations, he was in the habit of throwing the bridle upon his horse's neck, and reading as he rode. In this dangerous manner he travelled not less than 100,000 miles without any accident of importance. In his old age, however, he hurt himself upon the saddle, and was troubled with a hydrocele in consequence of the injury. For a long time he refused to have any thing done for his relief, but was, after two years, operated upon and cured. The following account of the operation is given in his journal.* "Mr. Wathen performed the operation, and drew off something more than half a pint of a thin,

* Journal xvii. p. 8.

yellow, transparent water; with this came out (to his no small surprise) a *pearl* of the size of a small shot, which he supposed might be one cause of the disorder, by occasioning a conflux of humours to the part." His biographer, Mr. Southey, after recording the circumstance, says: "I know not whether there be any other case recorded of physical *ostracism*."*

A case of somewhat the same kind is, however, related by Morgagni, as occurring in an old man with hydrocele. "*Scrotum animadversum est utrinque morbo non vacare. A dextris enim intra vaginalem tunicam, quæ non magis erat madida quam secundum naturam, calculos deprehendimus duos, undique solutus, alterum majusculum, alterum minimum, utrumque durum. Sinistra autem vaginalis, crassior facta, nullum calculum, sed aquam continebat multam, colore lixivium referentem.*" Morgagni de sed. et causis morb. Epist. LXIV. Art. 7. In this case the calculi did not seem to have any connexion with the hydrocele, as they were found on the side which contained no water.

Dr. Baillie speaks of loose cartilages in the cavity of the tunica vaginalis;† and several authors notice an ossification of this membrane as an occasional, but very rare, occurrence. I have searched almost all the standard works on Surgery and Pathological Anatomy, but, with the exception of the above case from Morgagni, I can find nothing similar to the one I have related. It has no particular interest in a practical point of view, unless so far as it may lead to the inquiry whether it was the cause or the consequence of the disease.

* The Life of Wesley, and the Rise and Progress of Methodism. By Robert Southey, Esq. New-York, 1820. vol. 2. p. 243.

† Morbid Anatomy, p. 348. 3d edition, London.

ART. IV. *Observations on the use of Arsenic.* By THOMAS D. MITCHELL, M. D. of Philadelphia.

It is not with the hope of determining the true mode by which arsenic operates in the cure of intermittents, that I have been induced to make the observations that follow. For after all that has been said on this subject, I believe that the difficulty of solution is as great as ever. I propose simply to state a few results of practical observation, as I believe that the truth can be attained only by a candid analysis of facts and by just conclusions deduced from legitimate premises.

In the autumn of 1820, I had very considerable opportunities for the exhibition of arsenic in intermittents. My residence at that time was in a village, (Norristown,) located on the margin of a river which had been lately dammed up in several parts, with a view of forming a lock-navigation. In consequence of the vast body of green timber employed for this object, and the immense quantity of water rendered almost stagnant by the erection of the dams, together with other causes, unnecessary to be detailed, almost the whole of the population suffered from ague and fever, a disease which had not prevailed in the neighbourhood for thirty years before.

When the disease made its first appearance I resorted to bark, with the confident expectation of being able to manage it by that medicine. But I soon discovered that it was not the proper remedy, although I did hear of a few individuals who cured themselves by taking an ounce at a dose. I did not long delay the exhibition of arsenic, which I used, chiefly, in the shape of Fowler's solution. I began with the customary doses, say from 8 to 10 drops three times a day for an adult, but this treatment did not profit my patients. Suspecting that the solution, which I had prepared myself, was not good, I sent to an apothecary in this city on whom I could depend, and procured a quantity which I expected would answer my purpose. But the exhibition of this produced no better effect than my own solution, although the dose was regularly increased in the usual way. At length, at the sug-

gestion of my friend, Dr. William Harris, of Chester county, I doubled the dose of the solution, and gave from fifteen to twenty drops three times a day, gradually augmenting the dose. The medicine now had the desired effect, and my patients in three or four days found themselves freed from the periodical attacks which had so much harassed them. I now viewed the solution of arsenic as a medicine that was by no means so dangerous as some persons had represented it to be. Possibly the disease was more violent in the vicinity in which I practised than it usually is, but certain I am that it was not curable by such doses of the solution as the late Dr. Barton was in the habit of recommending. I have frequently treated very young children, and old persons, with this article, and in no one instance did the remedy fail of arresting the disease; although, in many cases, owing to constant exposure to the miasmatic vicinity, the patients relapsed. My practice with children from two to five years old, was, to exhibit from three to six drops, three times a day; and never did I perceive any injurious effects to follow its exhibition.

To show still further the mistaken views of some, in relation to the very noxious properties of Fowler's solution, I will mention one or two additional circumstances that came under my notice. In the regular tour of visiting my patients, I stopped at the residence of a young man about twenty years old, for whom I had prescribed twenty drops of the solution three times a day, with directions to increase a drop daily. I called in just after he had swallowed the fourth dose, but soon discovered himself and family to be in great consternation, occasioned by an apprehension that the patient had poisoned himself, as some ill-natured practitioner in the vicinity had very officiously circulated a report that I was giving the people poison. I learnt that the young man, in the act of dropping his dose, was suddenly seized with a violent chill, which shook his whole frame so completely, that instead of getting 20 drops, he filled a tea-spoon of large size, and without pausing to consider whether it was proper or not, swallowed the whole of it. I determined to wait a while to see whether any bad consequences would follow, though by this time I had been so habituated to large doses, that I felt little or no

fears. After remaining from one to two hours, and discovering no unpleasant result, I left the patient, with directions to take no more of the medicine that day, and as he was costive, I prescribed a dose of Glauber salts. From that time he had no more chills, and was soon able to do his ordinary business. I had other patients who were extremely restive under the disease, and thought that if a little of the solution was good, more was better, and so repeated the dose, four, five, and six times a day; but in no one case did I perceive any inconvenience to follow: on the contrary, I believe that the disease was shortened in its duration. Nor was it in persons of gross habits and vigorous constitutions only, that I ventured to exhibit the solution in doses so unusually large. I have given it frequently to men and women of delicate frames, to infirm old men, and to puny children. And from the experience I have had in the administration of the article, I feel no scruples to assert, that it is one of the safest and best medicines for the cure of ague, that we possess.

My general mode of treating the disease was as follows. Finding, in almost every instance, that the stomach was disordered, more or less, I gave an emetic or an active mercurial purge in the commencement. When the patient complained of fixed pains in the right side or elsewhere, I drew blood once or oftener, according to circumstances. After the operation of the emetic or cathartic, if my visit happened an hour previous to the expected recurrence of the chill, I sometimes gave a grain or two of opium, and in this way succeeded in several cases, in breaking the chain of diseased associations entirely. But in most cases, I began with the solution immediately after the use of evacuant remedies. My practice was to moderate and shorten the cold fit, by applying to the back and thighs jugs of hot water or blocks of wood well boiled and wrapped in cloths, and to give, at the same time, warm but not stimulating drinks. As soon as the hot stage commenced, the clothes were lightened, cool drinks given, sinapisms applied to the ankles, and weak solutions of tartarized antimony administered just strong enough to nauseate. Thus the hot stage was shortened in duration, its violence mitigated, and a free perspiration induced on the whole surface.

With respect to the time of administering the solution, I am unable to say from experience that any particular direction was necessary, excepting to defer it when the stomach was sickened, until the irritation subsided. I have given it in the hot and in the cold stage; indeed it was impossible in some cases to avoid its exhibition in the hot stage, if given at all; because the febrile symptoms sometimes continued until within an hour or two of the recurrence of the cold stage. I gave the medicine, generally, in a little water or balm tea, and directed the diet to be light and of easy digestion. Occasionally I found it necessary to repeat the emetics and cathartics, and also the venesection.

It may be supposed that such frequent opportunities of administering the solution of arsenic in intermitting fevers, would enable me to offer a satisfactory solution of the mode by which this medicine produced its curative effects. But so far from this being the fact, it is true that the more patients I succeeded in curing, the more undetermined was my mind on this subject. This may appear from the following considerations, viz.

First, Some patients recovered, who had the œdematous swellings, which are regarded as peculiar to arsenic.

Second, Many got well, who had no swellings whatever.

Third, Some lost their chills and fevers, who were not sickened at any time by the medicine, and quite as many recovered who not only were made sick, but who occasionally threw up the medicine.

Fourth, Some patients had an increase of appetite, excepting when labouring under the hot or cold fit, and others lost all relish for food, so that bitters and tonics were necessary to restore the appetite.

All this variety of effect, I have witnessed repeatedly, and although my mind had been once decided as to the *modus operandi* of arsenic, yet the experience which furnished the above results, not only shook my faith, but placed me wholly in uncertainty. If I were to give an opinion on this subject, it would be, that arsenic does not possess a uniform mode of operation in curing intermitting fevers. That it does cure sometimes by inducing another disease, commonly called the arsenical œdema, is highly probable;

but that it does not always thus cure, is absolutely certain. I well remember one patient of vigorous constitution, who had been taking bark for a week or ten days without advice, and to no purpose, to whom I administered the solution of arsenic. He took but three twenty drop doses, and was completely cured, without the occurrence of a solitary symptom that could in any way explain how the cure was effected.

As a general rule, it happened that the patients who became œdematous were not sickened by the medicine, and vice versa. Now it may be, that while some of these were cured by the superinduction of œdema, the restoration of others was effected by some peculiar action on the stomach, which, sympathetically or otherwise, affected the whole system, so as to destroy the connexion of morbid associations. As to the tonic agency of arsenic, I confess no facts have ever come to my knowledge, that were calculated in the smallest degree to support the idea of such agency. In short, I am disposed to believe, that the mode by which arsenic cures intermitting fevers varies with the peculiarities of constitution in different persons, and that its remedial agency may not be exerted in the same way in all the attacks of the disease, even in the same individual.

As connected with this subject, I propose to make a few concluding reflections on the cause of the frequent recurrence of intermitting fever in the same person in successive seasons. For I do believe, that much on this score is attributed to the essential character of the disease which does not necessarily appertain to it, but is dependent on bad treatment. It is alleged, that a person who has once had the ague, is more liable to be attacked by it the following year than a person who has never had it; and in this way is it made out to be specifically different from yellow fever, which is said to attack the same individual but once in a life time. Now, although I admit the general proposition to be true, viz. that repetition of attacks is common to a person who has once had the disease, yet I deny that it is necessarily the case, or that this is, properly speaking, an attribute of intermitting fever. Phthisis pulmonalis often succeeds to an attack of pneumonia, and why? Does it necessarily follow that a person who has had

the latter disease must be the subject of the former? Such is not the fact. But when pneumonia is only half cured, when the inflammatory action is not sufficiently subdued, a foundation is often laid for phthisis, even in persons not so predisposed. Now I believe the same is true in relation to intermittent fever. Examine the people of low countries, and indeed in every situation where ague prevails, and observe how they manage the disease. As it occurs every year, they seldom think of obtaining medical advice, nor do they take medicine, ordinarily, until the disease has shaken their systems for several days. By this time some obstruction has been formed; or, as this is frequently co-existent with the disease, it has been greatly increased. They take one or two active emetics, or perhaps swallow an ounce of bark at a dose, and thus strangle the disease by involving it in a kind of suspended animation; for it is seldom absolutely extinguished in this way. If you look at these people, you at once perceive abundant proofs of the existence of hepatic obstructions, which, continuing from year to year, afford constant opportunities for the marsh effluvia to reproduce their baneful effects. For I feel confident, that the primary action of marsh miasmata is to derange the viscera, particularly the stomach and liver; and hence, that persons who have been improperly treated for the disease, and who consequently have obstructions throughout the year, are necessarily more liable to attacks than others who have never experienced an attack at all.

The position, then, which I wish to establish is, that intermittent fever, treated by bleeding, emetics, and active purging, so as to prevent or remove obstructions, may be so cured by arsenic, as to render the individual as little susceptible to future attacks of the disease, as a person who has never been the subject of its morbid influence. What is the practice of physicians generally in this disease? Why, their chief aim is to check the chills and fever, and thus enable the patient to attend to his ordinary business; and when this is done, they imagine their duty is discharged. But if they would save their patients from a repetition of the disease, or, at least, place them on an equal footing with those who

have never been attacked, they should deplete more copiously, and so prevent or entirely remove visceral obstructions.

On precisely similar principles is it, that persons who recover from yellow fever, are so seldom re-attacked. The man who survives a first attack of the disease, owes his life to the activity and violence of the measures adopted for his recovery. He is bled, purged, and otherwise depleted, to such an extent that all visceral obstruction is removed. His system is completely revolutionized, and the balance of excitability and excitement perfectly restored. Hence his health is in greater perfection than ever, and the vigour of all his organs is in exact equilibrium. But if it were possible for badly treated yellow fever patients to survive from year to year, and in consequence of such measures to be the subjects of extensive visceral obstructions, I have not a doubt that they would be more susceptible of the disease than are persons who have never been infected. These reflections are thrown out by way of hints to those who are in the habit of close thinking, in the hope that from an attentive survey of facts, a theory may at length be deduced, legitimate in its nature, and salutary in its consequences.

ART. V. *De Conceptu.* Auctore EDWIN A. ATLEE, M. D.
Philadelphiense.

IN quæstionibus Physiologicis, necessario obscuris, præsertim hæc, de conceptu; arrogans videtur ut ego, homulus, animadversiones ullas obtruderem: quoniam clarissimorum artis nostræ, non modo apud antiquos, sed etiam inter recentiores, inquisitiones occupavit; omnes vero, rem adhuc in tenebris atribus latere, fatentes.

Arcanum conceptus, unum ex quatuor mysteriis fuisse videtur, quod sapientissimus SOLOMON, non negavit, ultra perceptionem suam restitisse, nempe: "*Via maris cum virgine.*"

Quædam ex hypothesibus vetustis, mihi satis erit ut, causâ exordii, commemorem.

Pythagoras arbitrabatur, halitum, in coitu, de cerebro et nervis viri descendere, ex quo partes similes fœtus formari, et hanc progeniem, cursu 7, 9, vel 10 mensium, secundum leges harmoniæ, perfectam esse.

Empedocles opinatus est, aliquas partes fœtus in viro, aliquas in fœmina, contineri; et, ad prolem perficiendam, misturam necessariam esse.

Thesis *Galen*i fuit: Semen masculinum materiam tribuere, humorem vero fœmininum eam nutrire.

Harveius, qui in variis animalibus, progressum conceptionis indefessus insecutus est, credidit, quod velut ferrum per frictionem cum magnete, vim ejus peculiarem attrahendi participat, sic quoque uterus, tempore coitionis, vim plasticam embryonis concipiendi, comparat.

Recentiores, ut sentio, propius metæ accesserunt, quorum speculationes invicem defensores habuerunt: inter hos illustris *Haller* summam tenet. De sententiâ suâ, impregnatio ovuli matris, aurâ subtili fœcundâ seminis virilis, causata est. Ut impregnetur fœmina, necesse est ut semen intro uterum vel vaginam propulsum esset, et hinc aura ejus, per tubulos fallopianos, usque ad ovarium communicari posset. Opus est, quoque, ut ovum fœmininum maturum, et amplexu fimbriarum exceptum, fuerit.

Richerand doctrinam fœtus in ovario præexistentiæ restituit, conjectans eum per secretionem formari; et ovum, lineamenta novæ essentiæ capiens, de elaboratione sanguinis, per vasa spermatica, ad ovaria deducti, genitum esse: ovo autem adhuc inanimato semen masculinum, vi quadam electrica, ad id vivificandum, postulare.

Speculationes *postremæ*, jam nunc prævalide defensæ, duobus viris *claris* pro auctoribus potiuntur. Hæ denominatæ possint, *absorbens* et *sympathetica*. Harum posterior, in notis editioni novæ *Burnsii* de Obstetricio per Professorem *James* editæ, ad plenum inveniri potest; alter tam bene nota est, ut nihil attineat de illa in plagulis istis particulatim dicere.

Facile perceptu est, plerosque auctorum evolvendo, qui, post tempus *Halleri* scripserunt, quod ovaria in magno opere sobolis procreationis, necessaria sint; experimenta ultra controversiam

posuerunt, sterilitatem universe valere, ubi hæc vel desunt, vel labe afficiuntur. Est autem dissentio quoad *modum* impregnationis, non nulli affirmantes, semen, in orgasmati coitus, intro uterum propulsum esse, dum alteri concertant, structuram uteri vaginalis, impedimentum insuperabile contra ejus impulsionem esse.

Si judicium meum implicite quibusvis sententiarum præcedentium ductum esset, minima esset mihi hæsitatio, vel *Halleri* vel *Richerandi* opiniones adoptandi; atqui, “nullius addictus jurare in verba Magistri;” lubet doctrinam *magis liberam* advocare, nempe: *Semen masculinum non tantum intro uterum, sed quoque per tubulos fallopianos, usque ad ovarium impulsum esse*; haud ignarus, me contra pondus auctoritatis venerabilissimæ adversari.

Pro vindicatione doctrinæ hujus, argumenta *de conformatione genitalium utrimque* deducam, et confido ut evincere valeam, structuram partium, melius huic modo impregnationis mutuo adaptatam esse, quam istis per *absorptionem* et *partium consensum*; enim, utcumque speciosæ sint hæ duæ opiniones, confiteor me iis subscribere imparatum: quoniam, etsi creditu sit, *utrumvis possibile esse*, tamen nihil in structura genitalium mihi satis manifestum est, quo juste inferre possum, a priori, quod impregnatio, utroque modo, reapse eveniat.

Œconomia animalis, ea pars præsertim, quæ argumentum meum attinet, scrutatori accuratissimo demonstrat, coaptationem exquisitam formæ et functionis, destinationibus mutuis existere. Discimus quoque, ex scriptis eorum qui merite sagacissimi, patientissimi, et integerimi ex indagatoribus naturæ, dicuntur; scimusque nostris investigationibus, utut perfunctorie effectis, se nihil frustra agere.

Si mera applicatio seminis virilis ad vulvam foeminæ, per absorptionem sufficeret, nonne apparatus simpliciorum in sexibus plerumque obveniremus? Aut si *partium consensus* medium impregnationis esset, quid opus *membri virilis*, quid *corporum cavernosorum*, quæ turgiditatem, rigiditatem ejus causant? Quare *directio peculiaris*, media inter perpendicularem et transversam, cum liquidi fœcundi emissioni paratus est: quare secretio per *prostatam liquidi auxiliaris*, et *apparatus præpotens musculorum* ad illum projiciendum?

In conformatione *fœminæ* genitalium quoque, quid prodesset *vagina*, longitudine, diametro et *axe*, quibus universaliter prædita est? Quamobrem *os tinæ*, in hanc thecam, tumidis labiis, et *fissura transversa*, ut se *fissuræ verticali* penis aptet? Ut silentio transeamus *constrictores*, qui in membrum virile agere videntur, sicut manus lactariæ in vaccæ mamillam, ita seminis emissionem promovendo.

Cur, denique *vir totus* ad tales convulsiones ineffabiles excitaretur, lassitudinem, tristitiam, aliquando etiam mortem inferentes? Liberatio seminis, huncce tumultum non postulat; *imaginatio seipsa* huic adequata est. Absurdum est igitur, opinari, tam *futilem* eventum, per combinationem causarum tam *potentium*, effici. Nonne hoc aptissime similitudine ad *oceanum* accederetur, *procellis* vexatum, ad *plumulam* transmittendam, vel *muscam* demergendam? Ut loquitur poeta Anglicus,——

“—— resembles ocean into tempest toss'd,
“To waft a feather, or to drown a fly!”

Huc usque mihi videtur, ex principiis physiologicis deductum esse, quod etsi Natura aliquando in *anomalibus* ludat, tamen in hac provincia consummata (velut in omnibus ejus DIVINI AUCTORIS opificiis) instrumentum consilio proportionem æquans sit; dum plane multo majori vi prædita sit, quam defensores duarum speculationum postremarum admittere volunt. Etsi autem deductio mea non juste amoveri possit, tamen sunt, mentibus multorum, objectiones contra doctrinam. In his consistunt; nempe, 1. Quod, “etiamsi penis, ut nullo modo negari possit, vi impellente, alijus momenti, præditus sit, tamen, simul ac ista vis maxime necessaria sit, magnopere diminuta est, vel in toto suspensa, causâ firmitudinis qua, in vagina, amplectitur; insuper, quod densitas tenacitasque seminis causa moræ est, et rugæ vaginæ evidenter ad progressum hujus liquidi intercipiendum, destinantur.”

Ut his occurram, respondeo: Hæc effecta nunquam confirmata fuisse, argumentum igitur non æque assumptum esse. Videtur magis rationi congruens, opinari, hanc vim impellentem, causâ pigritiæ seminis, peni donatam esse, prostata interea fluidum uti

vehiculum secernenti; quinetiam rugas vaginæ ejus propulsionem auxiliari, non modo urethram firmando, sed quoque munere valvularum, regressum seminis obsistendo, istis rugis aliquatenus oblitteratis, dum vagina a pene distendatur, et formam pristinam recuperantibus, alternis membri virilis retractionibus, in punctis coitus suavissimis.

2. Objicitur, quod “etsi semen usque ad uterum impelli posset, non tamen intro cavum accepi potuisset, ob oris tincæ exiguitatem, et positionem, quæ haud directe in axi vaginæ, sed plus minus ad alterutrum parietem, vel ad sacrum inclinata est; aperturæ, idcirco, penis et uteri, non probabiliter in appositionem coiverint: insuper, quod labia oris tincæ plerumque tam firme agglutinata sint, ut prope coarctata esse. Canalis per cervicem et corpus uteri, pars ejus præcipue *angusta* dicta, usitato chirurgi exploratori magnitudine non æquet; glandulæ mucosæ et rugæ obstructionem augeant, immo etiam cavum nihil aliud videatur quam duo superficies se prope tangentes:” ut de amotionibus morbidis vel pravitatibus congenitis, sileam.

Hæ objectiones mihi nullius momenti apparent, argumentis ab utero *quiescente* deductis, igitur haud melius destinationi adaptato, quam esset *penis absque incitatione*, vel cordis *ventricula* expers sanguinis stimuli. Memorandum sit, quod etsi uterus arctus et compactus, est quoque *succulens, cellularis et muscularis*; fibra sunt mirifice intertextæ, *circumflexe* et *in longum* ordinata. His ita præmissis, nonne structura ejus proprie aptata ad amplificationem in tempore convulsivo impregnationis? Annon juste arbitrandum est, hanc amplificationem *puncto jucundissimo coitus* effici, potiusquam per ullum medium subsequens, vel *absorbens* vel *sympatheticum*, quod certe inadequum videtur renixum uteri superando? Positio peculiaris, supra dicta, *rimæ oris tincæ*, dum sit *transversa*, ad plenum ullam obliquitatem compensat, propter glandem penis; et semen, de *rima verticale* ejectum, necessaria imbibit.

Facto allegato, “impregnationis, ubi obliquitas vel prolapsus uteri talis fuit ut omnino transmissionem seminis impedivisse,” tantum respondeo; quod si hoc confirmatum sit, exemplum rarum præbet *virium systematis vicariarum*. Paulisper, tametsi, patien-

tiam lectoris tentabo, dum breviter exemplum adversum, quod sub notitia mea venit, narrem.

Uxor quædam consilium meum petebat, quæ, post primi infantis natum, prolapsu uteri per annos duos affecta fuerat. Examinatione inveni vaginam nonnullos pollices extra vulvam protrusam, fundum uteri sacro depressum, ore tincæ prorsus inaccesso. Fere inutile opinabar remedium tentare, nihilominus, cum illa, sine intermissione, a marito acerbe tractaretur, causâ sterilitatis (infante paulo post partum mortuo) strenuo me adurgebat. Ideo, post reductionem manu imperfectam, spongiam introduxi, jubens ut omni nocte amoveretur; injectiones astringentes mane et vespere uti; tantum quietis, positione recumbente, quantum occupatio sineret, usumque liberum roborantium interne.

Mensis circiter tribus. partes in pristinam positionem restitutæ fuerunt, paulo post hoc gravescebat, et tempestive puerum pulchrum edidit.

Ecce argumentum potens. Parentes certe progeniem diu mutuo desideraverunt; *superficies ampla diu absorptioni vel sympathiæ suppeditabatur, sine effectû. Causa frustrationis plane fuit, semen non intro uterum propulsum.*

3. Restat alter objectio, præcedentibus affinis; "Tubulos fallopianos tam peculiariter constructos esse, ut ne destinationi impregnationis, a semine introducto, adaptatos esse, aperturis tam minutissimis, canalibus gradatim dilatantibus, terminantibusque ore peramplo et patulo, id circo melius aptatis semen utero *asportando* quam *deportando*."

Breviter respondeo: Si conspectus meus accuratus sit, haud difficile est comprobari, diametra tubulorum necessarie, ob expansionem uteri subitam *dilatari*, sic libenter semen recipientes, quo semel recepto, renixus fere omnino cessare: canalibus dilatantibus, transitum haud frustrari posse; nam in positione usitatissima fœminæ, etiamsi nullus *motus vermicularis* in tubulis esset, per gravitationem, fere, semen fimbrias attingere posse, illic motus proprios excitandum ut ovariis applicaretur.

Pergere possem alias objectiones notandum, sed quoniam in præcedentibus volventur, iis vel stabunt vel cadent.

Dum materia hujus disquisitionis peculiaris sit, *linguam Latinam*, ne lectoribus indecorus æstimarer, selegi. Confiteor me adhuc longe a scopo abesse. Sint conspectus fallaces, monitum iis qui legerint, expecto, solícite postulans ut qui ad animadvertendum parati fuerint, lævitatem et obscenitatem caveant, quæ, meâ sententiâ, defectum sui respectus, si non sapientiæ, denotant.

Limites scientiæ nostræ revera circumscriptæ sunt, atqui *radio* tam vaste *extenso*, ut unicuique locus qui sufficit, præbeatur; in consecrationibus, licet viæ nostræ vel parallelæ sint, vel invicem intersecant aut adversentur; consideratio tamen, *utilitatem communem centrum esse*, debet, ut vinculum concordiæ, operari.

ART. VI. *A new Mode of Bandaging Fractures of the Clavicle.*
Proposed by Dr. STEPHEN BROWN, of New-York.

THE fracture of the clavicle is an injury of frequent occurrence. And although more bandages and apparatus have been invented, with a view of keeping the fractured extremities of this bone in coaptation, than perhaps for any other injury; yet, so inadequate are the most of them to this purpose, and so difficult of management are those most approved, that we frequently hear it said by surgeons of experience and skill, that they can perceive little advantage from any of them. Indeed, in injuries of this nature, many advise nothing more than to place the arm in a sling, and leave the bone to unite without any endeavour, by bandaging, to keep the fractured extremities in contact.

It is now known, that when the clavicle is broken the internal portion is firmly fixed in its place by the costo-clavicular ligaments, and by its being drawn in opposite directions by the sternocleido-mastoideus and pectoralis major muscles. The external fragment is always that which becomes displaced:—it is borne down by the weight of the arm and the action of the deltoid muscle; and being at the same time drawn forward and inward by the pectoralis major, is carried under the internal portion, which forms an eminence over it. If the fragments are left to unite in

this situation, the clavicle must frequently become shortened, and the muscles attached to it somewhat deranged; and it is reasonable to suppose that the shoulder of that side will not bear the same free motion as before, but will be susceptible of weakness and pains for a length of time, if not for ever afterwards. Experience confirms this fact. When a lad, I had the misfortune to fracture the clavicle of the left shoulder. It was managed with the figure of 8 bandage, which every experienced surgeon of the present day, perhaps, will allow cannot be applied so as to answer the purpose intended. The consequence of the injury was, that it occasioned pain and weakness in that shoulder for several years afterwards, especially after free exercise. I have seen other instances where the use of the arm has become impaired from the fracture of this bone.

Again, if there should be considerable laceration of the soft parts, and the extremities of the bone could not be kept in contact, it would be an impediment to the healing of the flesh wound, and possibly caries of the bone might supervene.

Previous to the time of Desault, the methods of managing the fractured clavicle were by the bandage in the form of the figure of 8, the iron cross of Heister, the corselet described by Brasdor, and the leather strap proposed by Brunninghaussen. These latter were intended as improvements, but have been supposed to be only modifications of the figure of 8 bandage, and appear to have no advantage over it. It is unnecessary to notice the defects of these methods in accomplishing the indications of cure, since they have fallen into disuse.

Desault first suggested the proper indications in the reduction of the fracture of the clavicle: and invented an apparatus,* which, in his hands, appears to have been very successful in the management of this injury. But the success of surgeons generally in the employment of Desault's method, is not so complete as it appears to have been with him. The apparatus of Desault has every indication of cure, but his mode of bandaging has many disadvantages, some of which we will endeavour to notice.

* For a description of this apparatus, I beg leave to refer the reader to the *Treatise on Fractures and Luxations*, by P. J. Desault.

1st. It consists of so many casts around the body, that it is almost impossible to apply it in such a manner as to prevent its becoming, in a short time, so deranged as to require to be taken off and re-applied.

2d. In females this bandage is peculiarly inconvenient, not only from the greater difficulty of applying it, but from its pressure on the mammæ, rendering it at least very troublesome if not injurious.

3d. In both males and females, when applied so tight as fully to answer the purpose intended, it renders respiration difficult, and frequently, in asthmatics and persons whose lungs are predisposed to be affected, it cannot be endured.

4th. It covers the injured clavicle; and in cases where local dressings are required, as in wounds of the soft parts, it will be necessary to remove a part of the bandage for their application.

It has also the disadvantage of being difficult of application in all cases, from the number of pieces of which it consists, and the length of time required to make the application; and whenever it is deranged it becomes necessary, in order to re-apply it, to raise the arm and move the shoulder, which must occasion an impediment to the speedy consolidation of the fracture. Boyer has invented another apparatus,* more simple than that of Desault; but it appears liable to some of the objections we have made to his, and we think it not well calculated to accomplish one of the most important indications in the treatment of these fractures, that of raising and supporting the arm.

The plan of Mr. Charles Bell, by means of the double-headed roller and compress under the arm,† seems better adapted to general application than that of Desault or Boyer. It has none of the inconveniences we have attributed to theirs. But, as the force exerted to keep the external portion raised in contact with the internal, depends upon the short turns around the shoulder, a very slight stretching of the bandage will occasion it to be displaced; and unless the bandage be frequently re-applied, it cannot be retained in its situation.

* See the Treatise on Surgical Diseases, by baron Boyer.

† See Bell's Operative Surgery.

During my connexion with the surgical department of the New-York Alms-House, several cases of fracture of the clavicle fell under my observation and management. After trying the most approved methods which have been recommended for the treatment of this injury, I was led to the conclusion that they were all defective or faulty; and was induced to try some new mode, with a view to keep the fractured extremities in coaptation with the least inconvenience to the patient. After repeated trials, I have adopted the following plan.

Take a single-headed roller, eleven yards long and three and a half inches broad,* and place one end of it a little forward of the axilla of the opposite side; carry the roller from thence across the upper part of the chest under the armpit of the affected side, and around the body to meet the end, over which let it lap a little, and pin or sew it fast. Place a cushion in the armpit, as directed by Desault, which is to be attached to the bandage thus passed around the body by tapes, or, which is better, let it be sewed. The cushion being fixed, the surgeon seizes the patient's elbow, the forearm being bent to a little less than a right angle with the arm, and brings it forward, upward, and inward, pressing it closely against the body. Let the shoulder be sufficiently raised, so that the external fragment be carried up to its place, and the deformity entirely disappear. Let an assistant hold the elbow and forearm in this situation, while the surgeon brings the bandage down obliquely across the breast over the forearm nearly across its middle, passing it around under the elbow and across the back obliquely upward to the lower part of the scapula of the opposite side. Here it should be pinned or sewed to that part which was first passed around the body; then with a turn carry it over the shoulder of the same side, and down obliquely across the breast, as before, overlapping the first cast about two-thirds, and across the forearm, nearer the elbow than the first; carrying it around under the elbow, and up across the back, to be fastened upon the preceding cast in the same situation where that was attached to the first. With a turn carry it over the shoulder, where

* A bandage narrower than this will, of course, be found more convenient in cases of children.

it should nearly cover the preceding cast; then down across the breast as before, overlapping the preceding fold as the second does the first. Four or five of these casts over the forearm and elbow are sufficient. Let the last two or three embrace and support the elbow, with such a degree of force as to keep the shoulder well raised. Fasten the cast which was last carried around to as many folds of the preceding ones as it will cover, just forward of the elbow, upon the forearm;—then with a turn carry it over the arm just above the elbow, across the back, under the axilla of the opposite side, and around and across the chest, to be attached to the folds upon the forearm, as before. Then with a turn overlap the first cast, and carry it around in the same manner.

Two or three of these casts are sufficient; the object of which is to keep the lower extremity of the humerus closely in contact with the body, which, by the aid of the cushion, keeps the shoulder outward.* The hand may be supported in a sling formed of the last extremity of the bandage, the last cast of which may be pinned to the folds upon the back, and the end brought over the shoulder to the wrist; or, attached to the folds upon the breast, and the end passed around the wrist and hand once or twice, and fastened again.

Before applying the bandage, a cushion of several folds of soft linen should be laid upon the shoulder to prevent irritation, over which the folds of the bandage should rest. To answer the same purpose, the arm and forearm may be covered with a layer of soft linen, and a few folds of the same placed in the armpit of the opposite side.

The bandage being applied, let the casts, where they overlap, be sewed to each other in every situation where the bandage is

* In females, in order to obviate, as much as possible, any inconvenience by pressure upon the mammæ, the casts which are carried over the humerus, when they are brought under the armpit of the opposite side, instead of being continued directly across the breast to the lower part of the humerus of the affected side, may be carried obliquely upward toward the neck, upon the casts that were passed over the shoulder; to which let them be fastened. Then with a half turn continue down upon the forearm near the elbow, and fasten again; then with a turn carry it over the lower part of the humerus, &c. &c.

most likely to be deranged. This is particularly necessary where they cover the arm and forearm.

This mode of bandaging will be found to possess equal facilities with the most approved plan of reducing fractures of the lower extremities.

The first application of this method was in the case of a young man about twenty years of age, the second day after the accident. The bandage was worn nearly four weeks; at the expiration of which time the fracture had united without deformity; the shoulder continuing throughout as well raised and supported as when it was first applied. The second application was in the case of a female about the same age, with similar success. The third was a child about five years of age. In this instance the bandage became a little deranged, and was re-applied on the seventh day; after which it remained without any re-application or adjustment till the cure was completed.

The advantages of this bandage are,

- 1st, Its being simple, and requiring but little time for its application.
- 2dly, If well applied, it will rarely become deranged, so as to need a re-application.
- 3dly, It occasions little inconvenience to the female, as it can be applied without any pressure on the *mammæ*, except slightly on one side.
- 4thly, It does not compress the chest so as at all to obstruct a free respiration.
- 5thly, No part of it comes in contact with the injured clavicle or shoulder—so that if any local application is required to a flesh wound, or to allay inflammation, it can be made with the greatest facility.
- 6thly, Should it become deranged and require a re-application, this can be done without moving the arm or shoulder.
- 7thly, It confines the arm of the injured side only, the other remaining entirely at liberty.

The following cuts will assist in conveying a true idea of this bandage to the reader's mind.

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Fig. 1.

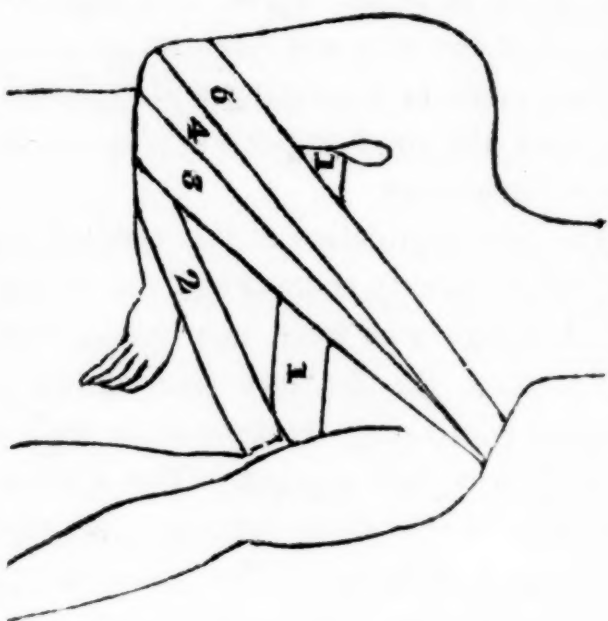


Fig. 2.

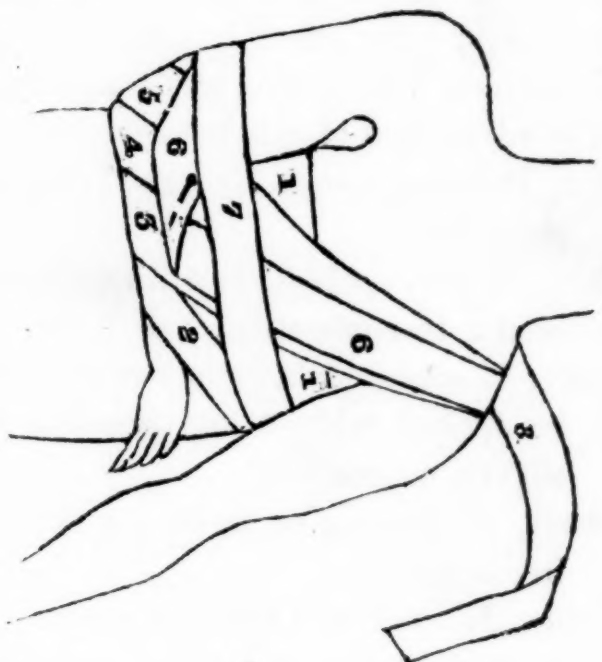


Fig. 3.



H.W.D. del.

FIG. 1. Represents the first and second layers of the roller; and designates its several turns by figures, in the order in which they are made.

FIG. 2. Represents the bandage complete; and especially the turning of the roller to bind the arm firmly to the body. It also shows the manner in which the tail of the bandage is to be brought over the shoulder to form a sling.

FIG. 3. Represents the arm in the sling.

ART. VII. On the Use of Red-Precipitate Ointment in the Treatment of Ophthalmia. By L. P. GEBHARD, M. D. of Philadelphia.

CASE I.

IN the spring of 1817 I was requested to visit Mrs. P—, aged about 50 years. I found her labouring under a pulmonary affection, and after having attended her for several months, a violent inflammation suddenly attacked both her eyes. This resisted the mode of treatment usually employed in such cases; her system being very much reduced by a protracted pulmonary affection, general bleeding was considered inadmissible, and topical bleeding by cups or leeches being strenuously opposed by her, I had recourse to blisters on the temples, kept irritated for a considerable time by citrine ointment. These, together with collyrea of different kinds, as *cerus. acetat. sulp. zinc. nit. pot. brandy* and water, rose water, &c. were employed, and with no perceptible advantage. Believing the case to be such as would bear a stimulating application still more powerful than any that had been employed, and thinking an ointment to be a better form of application than any other, I prescribed the *unguentum rub. precip.** to be introduced into the eye on the tip of the finger, or with a camel's hair pencil, about six times a day. This far exceeded any expectations that I at that time could have had even of the most successful remedies generally employed in the disease. It is a remedy which I confess appears to be a pretty harsh one, and which I had never read of having been made use of in such a case; but it is, I conceive, a duty incumbent on every practitioner, when any perplexing case occurs which has baffled him at almost every point, and resisted nearly all the remedies usually employed in that disease, no longer to adhere with pertinacity to

* The ointment used is made thus :

| | | |
|---|----------------|--------|
| R | Præcipit. rub. | gr. 30 |
| | Axung. porcin. | ʒi. |
| | Tere simul. | |

the beaten track. Scarcely had the ointment been applied to the eye for twenty-four hours, before evident signs of amendment appeared; and in the short space of five or six days, her eyes were entirely restored; not a particle of inflammation remaining. In this case the system was evidently much below the healthy grade of excitement; having been reduced to this state by the long protracted phthisis, and the strict antiphlogistic plan which had previously been employed to remove all the inflammatory action from the system; so that, even during the hectic stage, her pulse bore strong marks of debility upon it, and a tonic plan was at this time required, and continued throughout the remaining part of the disease.

CASE II.

The next case of tedious chronic ophthalmia that fell under my notice was in December 1818; it occurred in a child between two and three years of age, the daughter of S—— R——. The common refrigerant applications were employed, and in a short time her eyes became better, and continued so until about the 20th of January 1819, when, having been exposed to cold and damp weather, the disease returned with redoubled violence. The case was now attended with considerable fever, which was effectually reduced by a rigid antiphlogistic plan of treatment. An artificial diarrhœa was kept up by the constant use of sulph. soda for several weeks; notwithstanding this, and the long continued use of different applications to the eyes, such as collyrea of sac. sat. sulph. zinc, tinct. opii, rose water, brandy and water, &c. together with the application of leeches to the eyes, and blisters to the back of the ears, which were kept irritated for a length of time by the application of citrine ointment to them, at the end of nearly six weeks the state of her eyes was, to all appearance, not in the least improved; she was, nevertheless, during the whole of the time confined to a dark room, living altogether upon the simple fare of bread and water. Having failed thus far in relieving the inflammation, the mother having been necessarily confined to the room with her child, became so tired and impatient with the confinement and

discouraged with the want of success of the remedies already employed, that she seemed determined soon to break through all the restrictions that had been so long imposed upon her child, and necessarily upon herself; and, as she expressed it, once more to be enabled to behold the light of day. Being thus perplexed on the one hand with the dissatisfaction of the mother, and on the other with the obstinacy of the disease, I determined again to try the ung. rub. precip. As this case was a chronic one, and similar to the former, I thought the remedy equally applicable in both cases, and I was happy to find the same pleasing and satisfactory results follow its use in both instances. An unfavourable circumstance attended the use of the ointment in this case, namely, the child having been introduced to the light about the time of its first application; but, notwithstanding all this, the eyes mended rapidly. On the second day after its first application an improvement was very perceptible, and in the space of ten or twelve days her eyes were restored to their usual vivacity and health. Since that time she has had several slight attacks of the disease upon exposure to cold, and in every instance the inflammation yielded to the application in two or three days. A sister older than this patient was relieved in a recent case by the same application.

CASE III.

The daughter of M—— B——, aged two years, was seized with an inflammatory affection of both eyes, in the month of April, 1819. Several weeks had elapsed before I saw her. Upon examination I found a speck of considerable size on the cornea of one eye near the margin of the sclerotica, extending nearly opposite the pupil. The cornea was considerably projected, and swelled at that place; and the whole surface of the eye was apparently very much inflamed. A dark room and low diet was ordered, and I prescribed refrigerants, as, sup. tart. pot. and sulp. sod. to be taken very liberally, to relieve the febrile action, and immediately applied the ung. rub. precip. to the eye. In the course of a few days the whole

surface of the cornea became entirely cleared of disease, while the conjunctiva was also much improved in appearance. She then, by being imprudently exposed to the light, as well as to the air, again contracted the disease; the febrile action returned with redoubled violence, and the eyes became much more inflamed. The refrigerants, as recommended above, were again employed, with an addition of jalap to the sup. tart. pot. The mother had applied a blister to the back of one ear previously to my being called the last time; but it was attended with no manifest advantage. The ung. rub. precip. was again employed, and effected a cure in a very short space of time after its application. I was induced to make use of the ointment in this case, from the success which attended its use in the mild recent cases already mentioned.

CASE IV.

In the autumn of 1819, a child of Mrs. R—— was seized with ophthalmia in one eye, attended with a fever of the remittent kind, almost immediately succeeding the measles. Ordered sulph. sod. as a cathartic, spts. nit. dulc. as a refrigerant, and sup. tart. pot. for a constant drink; at the same time making use of the ung. oxyd rub. hydrargyri as an application to the eye, which, in the course of a few days effected a perfect cure. Low diet, and a dark room, were at the same time strictly enjoined.

CASE V.

M—— C——, aged about thirty-five, case of ophthalmia in one eye, cured in a few days by the application of the ung. rub. oxyd. hydrarg. to the eye. As there was no febrile action attending, he was enabled to attend to his out door business throughout the whole time. A moderate diet, and a dose or two of salts, were all else that was done in the case.

CASE VI.

Many other similar cases might be mentioned. I shall however relate but one more. E—— B——, son of C—— B——, aged two years and a half, was attacked with measles in the be-

ginning of February, 1820, which was succeeded by an inflammation in one eye. I ordered the ung. oxyd. hydrarg. rub. which had the effect in a few days of producing a great improvement in the appearance of the eye. The ointment now being neglected, and the child left to run out of doors, and indulge freely in all kinds of high-seasoned animal food, he soon relapsed into a much worse state than the first. After this it was observed that the inflammation had spread nearly over the whole surface of the eye, producing an opacity of the cornea, immediately before the pupil, about the size of a fennel seed. I ordered his eye to be leeches immediately, and gave him cathartics alternately of ol. ricini, sulp. sod. senna, sup. tart. pot and sal, the last two of which were continued for some time, producing copious evacuations from the bowels. About a week after the first leeching the leeches were again applied. Dr. Physick was now called in consultation; he recommended four grains calomel, and the same treatment to be continued, with respect to diet and darkness. After this he was bled from his arm, and the antiphlogistic plan continued for four weeks longer, when it was found, that the speck on the cornea had somewhat diminished, notwithstanding which the inflammation of the conjunctiva continued without any abatement, until the middle of April, when, finding the collyrea composed of such articles as sac. sat. sulp. zinc, sulp. cupri, &c. were of no avail, I again prescribed the ung. rub. hydrarg. oxyd. which entirely eradicated the inflammation in less than two weeks.

As early as 1765, it was the opinion of Dr. Vacca, an Italian physician, that in inflammation the action of the capillary vessels instead of being increased is diminished, and consequently is dependent on debility. In 1790, Mr. Allen and Dr. Lubbock defended this theory before the Medical Society of Edinburgh; soon after which Dr. A. P. Wilson Philip instituted a course of experiments on the web of a frog's foot, followed recently by those of Mr. Hastings, which go to prove, in confirmation of the

opinion of Vacca, that upon inflaming the web, the action of the vessels began to diminish; and as the inflammation progressed, the action lessened, until at last the circulation became almost stationary. It was also observed, as the inflammation proceeded, the vessels enlarged, the globules in the circulating fluid disappeared, and vessels which from their size had before refused to admit any other than an almost colourless fluid within their calibers, now yielded apparently without offering any resistance to the impetus of the *vis a tergo*, and the admission of red blood was the consequence.

After these satisfactory experiments, proving incontestably, that inflammation is actually founded upon the debility of the capillaries, Mr. Hastings endeavours to point out the distinction between active and passive inflammation; which he thinks consists in the degree in which the arteries supplying the blood to the debilitated vessels are excited. But why, I would ask, is this distinction to be made, after it is so clearly proved by experiments that debility is not only the sole cause of, but is also co-existent with inflammation throughout every stage of it, from beginning to end? And what have we to do with the state of the arteries that supply these debilitated vessels? I contend that the state of the system, whether it be above or below par, has nothing to do with the production of the inflammation. It is not the increased action of the whole arterial system that necessarily makes an inflammation, locally situated, active; nor is it actually productive of inflammation.

Inflammation, if I understand right, consists in the preternatural enlargement of the debilitated vessels, and is totally independent of, and opposed to, the state of the system. In illustration of the subject, I would observe that the inflammation is as independent of the system as a menial slave is independent of his master. Not independent of in point of power or authority, (for the term master in itself signifies a superior, and that of a slave, a dependant, or inferior); but independent of in point of situation or estate. Is the master in an active state, compared to the slave? so is the system, compared to the inflammation. Is the slave in a passive state? so is the inflammation. Is the slave subordinate to, and his state

directly opposed to that of his master? so is the inflammation subordinate to, and the state of it directly opposed to the state of the system. The balance of action, or power between the system and the inflammation, is entirely destroyed; and not necessarily because the system has assumed more than her just power, but because the inflamed part has lost its power, and yielded without offering the usual resistance to the natural impetus of the blood; therefore, the state of the system may be no higher than natural, yet the balance of power may preponderate greatly in favour of the system, because there is a great deficit of action in the inflamed part, by reason of debility. But it may be said that this doctrine is a dangerous one; it might lead to serious consequences. This I am not willing to admit. Though I have endeavoured to make it appear, that the state of the inflammation is directly opposed to, and different from, the state of the system, yet I would not have any one for a moment to suppose, that because the system has the inflammation in subjection to it, it should be allowed to retain that power. But instead of bringing down the whole system to an equilibrium of action with the part affected, I would raise the part to an equal standing with the whole. On the contrary, however, if the system has acquired too great a power; in other words, if it be raised above its natural standard, it is the duty of the surgeon to act the part of a Delilah; to deprive this Samson of his strength, and humble him to his former station; at the same time lending a supporting hand to the enfeebled vessels of the part inflamed, and restore them to an equal grade of tone with the general circulation.

I wish now, for a few moments, to direct the attention of the reader to a few remarks more particularly on the inflammation of the eye.

It will be observed by the cases related, that the ointment was made use of both when the system was above and below the healthy grade of excitement; and it will also be seen, that the efficacy of the application was not in the least influenced by the different states of the system. I would infer from this, that inflammation of the eye, from its very commencement, is not attended with a strong, but a weak, morbid action in the capillaries;

in other words, that the vessels are not in an active, but in a passive state of excitement; and this is evident from the efficacy which has attended the stimulating applications, whenever they have been employed.

It must be confessed that we know but little of the *modus operandi* of medicine; but notwithstanding, it must be admitted, that, judging from the symptoms, and the effects that medicines produce upon the system in removing those symptoms, we necessarily must have some just and adequate conceptions of their action upon the system; and this we may still further infer from our having established principles and rules to govern our practice in many cases: for instance, if we were called to prescribe in a case of acute inflammatory action of the system, would we not immediately deplete according to the state of the system, and make use of a general antiphlogistic treatment? Most assuredly. But the cases just related, though they were generally treated upon the antiphlogistic plan, the stimulating application was made use of in several cases, from the very commencement of the disease, while the heat, redness, pain and sensibility to light, existed in full force; and all these symptoms, instead of becoming more aggravated by the stimulating application, (which necessarily would have been the case, had the inflammation been of the acute kind, or attended with a strong morbid action), in a few days entirely ceased. This is certainly the strongest evidence that can be given of the passive, or weak morbid action of the inflamed vessels.

I know it has been a general practice to make use of stimulating applications, in the chronic or passive state of inflammation; and hence it has been considered conclusive, that when stimulating applications have resulted in the cure of inflammation, that that inflammation must necessarily have been attended with a weak morbid action of the capillary vessels. If this be the case, I ask what inference is to be drawn from these facts? It is namely this, that inflammation, whether recent or chronic, is of the passive kind. But if the inflammation be acute, or of the active kind, as is supposed, at the commencement of the disease, the action of the vessels must necessarily change, during some period of the disease, to a passive state. Now, who will pretend to draw a line of de-

marcation between these two states of inflammation? No practitioner can consistently do it, as his practice will teach him to the contrary. It is this practice that will, sooner or later, erase from the mind of the practitioner that imaginary line which has so long been drawn between active and passive inflammation. It is this practice, that knows no change of vessels, no change of action, no change of symptoms; but finds the disease to be the same from beginning to end. It is true that Scarpa tells us, the redness of the eyes may continue for some time after the ophthalmia has relapsed from its acute stage into that attended with relaxation of the vessels. I admit this in part; that is, that the redness continues from the beginning to the end of the inflammation. But have we any reason to believe that a disease changes, or rather that the action of the inflamed vessels changes, while the symptoms remain the same? Now, in several of the cases that I have related, I observed the same heat, redness, pain, and sensibility to light, even after the third and fourth week from the attack; during the whole of which time, the patient was undergoing a course of depletion, as fast as the state of the system would permit, as I observed at the commencement of the disease: and even after the system has been reduced below the healthy grade of excitement, I have observed the same appearances of the eye continue.

Now, I am unwilling to resign the position that has been taken, until it can be satisfactorily proved, upon what principle stimulating remedies can relieve the action of highly inflamed vessels, of the acute kind, by their application to them. Here, I think, it will be at once conceded, that this position is by far the most tenable. Admit this, and the *modus operandi* of the remedies employed is at once accounted for. If it be admitted that the vessels are weak and relaxed, the stimulating and astringent applications at once strengthen and lessen the diameter of the inflamed vessels, thereby enabling the vessels to get rid of the superabundance of blood, by restoring them to a healthy action.

But it may be said that it is inconsistent with reason, to suppose that the vessels of the eye can be in a minus, while the whole sanguiferous system beside is in a plus state of excitement. This,

however, I think, may be very easily accounted for. It is well known, that debility is generally the predisposing cause of disease. Supposing this to be the case with the vessels of the eye, and that this action has been induced by too much exercise of the eye, or from other causes: we have now, therefore, the remote and predisposing causes. We will now suppose cold to be the third, or exciting cause, and that this cold affects the whole system at the same time. This being the case, the system at once rises above the healthy point of excitement, and pours a volume of blood into these weak and unprotected vessels. Here the eyes are left in a dilemma, from which they have not the ability to extricate themselves. In this weak and unprotected state, they are unable to resist the constant and unceasing encroachments of the system, and, consequently, are compelled to yield. But the skilful surgeon arrives; and while he is disarming the system of its power, he is at the same time granting support to the enfeebled vessels, by which an equilibrium is soon restored, the inflammation disappears, and health is the consequence.

ART. VIII. *Observations on the various Forms of Conjunctivitis.*
By GEORGE FRICK, M. D. of Baltimore.

THE term ophthalmia was employed by the older writers to include all inflammations of the eye, without any distinction as to the particular tissue or structure which was the immediate seat of the affection. It is true, in looking into some of these writers, we find very frequent mention made of an internal ophthalmia, or of a division of ophthalmia into external and internal: but none of these have pointed out any precise or correct diagnosis; or drawn from this distinction any just or rational indications of cure. Professor Schmidt, an eminent teacher in the school of Vienna, in a work on Iritis, has been the first to draw the attention of the profession to this very important and interesting branch of research: and since his time, considerable improvements have been made by his successors in this department of pathology. The Germans,

perhaps, are of all nations those who excel in nice discrimination of the various forms of disease; and they appear to have carried this discrimination, in the subject now under consideration, to its utmost extent.

Nothing is more certain, than that every acute inflammation of an organ begins in a single tissue of its structure, and is accompanied by a set of phenomena, indicative of the peculiar tissue affected; that where this inflammation extends to the adjacent tissues, it is attended with another train of symptoms, alike indicative of the structure newly invaded: yet how common is it to confound these different inflammations, and to speak of the inflammation of an organ, as if the disease attacked, at one and the same time, the whole of the tissues which enter into its organization?

It is a little remarkable, that in the various treatises on inflammation, a subject which has occupied the time and attention of so many eminent pathologists, no attempt has been made to classify the different varieties of the disease, according to the individual textures of the body in which it is situated. Dr. Thompson, in his very valuable work on inflammation, has merely hinted at the subject, without considering at all its importance. And the more modern French writers, among whom I may mention Broussais, Boyer, Richerand and others, have taken no notice of it. Pinel, I believe, is the only author who has adopted this classification; but his system is by no means complete. He has enumerated merely the inflammations of the cutaneous, mucous, serous, cellular, muscular, fibrous, and synovial systems, without taking into view others equally important; and he has treated these in a general way only, as so many forms of inflammation, without attempting to describe the peculiar symptoms, progress, periods, products. In a word, he has not given us the characteristics of these species. Bricheteau, in the article *inflammation*, of the *Dictionnaire des Sciences Medicales*, has followed Pinel in his classification, and has added to the consideration of those species noticed by this gentleman, several others; as the inflammation of the nervous, sanguineous, absorbent, and osseous

systems. The subject, however, is not treated with that depth and acumen its importance merits.

In the following remarks, I shall confine myself entirely to the consideration of inflammation, as it invades that portion of the mucous structure of the body which envelopes the eye. The subject, though very nearly exhausted by the labours of many learned pathologists of the Continent, will, the author believes, prove entirely novel to most of his readers on this side of the Atlantic.

The most simple form under which inflammation attacks the conjunctiva, is that of *catarrhal ophthalmia*, or what is denominated by the English ophthalmologists the *puriform ophthalmia*. The disease commences, most commonly, with a sensation of itching, or burning and stiffness in the borders* of the eye-lids. The eye is dry, and less moveable in its socket. On examining the conjunctiva, we shall find the redness unequally diffused over its surface, and the vessels running in distinct bundles, or fasciculi, across the eye, broader in the corners where they seem to originate, and gradually diminishing in size as they reach the cornea. That the inflammation is seated in the conjunctiva, and not in the sclerotic, is evident from the size of these vessels, from their being more deeply coloured, and from their lying more superficially. They swell and subside again more rapidly than those of the sclerotic. In the motions of the eye itself, they remain stationary; but when the eye-lids are moved, they are observed to follow.

It is this particular enlargement of the conjunctival vessels that causes the sensation as if grains of sand, or other extraneous matter, were lodged upon the eye.

The disease, like all other species of catarrh, has its exacerbations in the evening; and this is sufficient to distinguish it from

* If inflammation consist, as Dr. Grattan supposes, (Vid. Transactions of the Irish Association, Vol. II.), in a disproportionate action between the capillaries and minute arteries, the reason will be obvious why the disease commences, as I have stated, in the borders of the eye-lids; as Sæmmering has beautifully shown, in his anatomy of the eye, the finer communications between these two orders of vessels are precisely at this part.

the strumous ophthalmia, especially in children, which is generally worse through the day, and remits at night.

It is generally connected too with inflammation of some of the other mucous membranes, and like these, when violent, is ushered in with fever. The mucous lining of the bronchiæ, which may be said to constitute an integrant part of the conjunctiva, is more commonly affected; sometimes it is the Schniderian membrane of the nose, and its continuation into the frontal sinuses, or antrum of the mouth, which accounts for the violent pain that is often experienced in these parts, under attacks of this ophthalmia. It may happen, too, that this membrane is the primary seat of the inflammation, and that the conjunctiva is only affected secondarily, or from the sudden suppression of disease here. We have instances of the latter in the case of measles, scarlatina, &c. where they reign epidemically.

The inflammation, in some cases, extends externally over the integuments, and the skin is excoriated from the very acrid secretions which are constantly overflowing it. At the end of four or five days, a mucous or puriform secretion takes place from the lids. "It is the thin tender membrane which thus forms over the cornea, that causes the patient, in this stage of the disease, to see all objects as enveloped in a mist; and, if he looks at the candle by night, he sees it but dimly, or as surrounded by a circular zone or iris. These appearances alarm him exceedingly at first; but, upon moving the eye-lids rapidly and frequently across the eye, or upon washing or wiping his eye, he finds them to vanish completely, as he has removed the flakes of mucus which caused the delusion."* Pustules of matter likewise, scarcely visible to the naked eye, form along the margin of the eye-lids, or on some part of the conjunctiva, which, bursting, pour forth their contents with the other secretions. Some of these contain nothing but a yellow serum; others are filled with a more purulent-like fluid. Both kinds are often dissipated by proper management; most commonly they burst, and, discharging their contents, leave behind small superficial ulcers. These vesicles, which are

* Beer's *Lehre von den Augenkrankheiten*. Band I, p. 289. Wien, 1813.

probably nothing more than the inflamed mucous glands of the conjunctiva, are very analogous to the aphthæ which are so frequently met with in the cavity of the mouth, on the lips, tongue, internal surface of the alimentary canal, and other mucous membranes of the body. "As some remarks of Professor Himly illustrate very strikingly this analogy, I shall quote his words. At a time when aphthæ of the throat were very frequent at Brunswick, I also found many vesicles, beginning with an inflammation of the sclerotic coat; and also sometimes, but more rarely, of the cornea. Once, I saw a whole family affected with this disease, one after another. It was a true catarrhal affection; and in some cases, these vesicles disappeared by diaphoretic medicines, in some by blisters, camphor and antimony, without any local application, except mucilaginous ones. I think it is just the same disease as aphthæ of the intestinal canal, of the corona of the glans penis, and other fine continuations of the external skin."*

The catarrhal ophthalmia is a disease very common in strumous habits, and is generally attended with a disordered state of the general health. Hence it may be said to be in a great degree a constitutional affection. It is most frequently accompanied too by disorder of the other mucous membranes. It is most common in the lower classes of persons; in the children of such as are badly nourished; in individuals exposed to animal effluvia,† &c. No class of persons, however, are entirely exempt from its attack, and we find it in all ages and sexes. Professor Schmidt supposes such persons are more particularly liable to attacks of this disease, who have very 'red cinnabar cheeks, with a shining, parchment-like skin,' or as he more emphatically expresses himself, such individuals, of whom it is said, they look as if constituted of 'blood and milk.'‡

The disease is known at times to reign endemically in whole

* Vid. Loder's Journal, Vol. I. or Wardrop's Essays on the Morbid Anatomy of the Human Eye, p. 42.

† A variety of this ophthalmia, called in France *la mite*, is very common among nightmen: which M. Dupuytren attributes to the action of the hydro-sulphuret of ammonia.

‡ Vid. Ophthalmologische Bibliothek, Band III. S. I. Hanover, 1805.

families, schools, &c. and this has given rise to a very general opinion among the English surgeons, of its being contagious. Like other diseases of the mucous membranes, as catarrh, &c. the disease is found to exist epidemically in towns or through large districts of country, depending, no doubt, upon certain changes in the constitution of the atmosphere. Sometimes it is found to prevail at the same time with an epidemic influenza; and it is very remarkable that all those persons attacked with ophthalmia, are exempted from the influenza. Physicians have commonly accounted for this circumstance, upon the principle so generally admitted among the laws of the constitution, viz —that one disease supersedes or prevents another from taking place at the same time in the system. It is more rational to suppose that, in this instance, the disease is one and the same, attacking in both cases the mucous structure of the body. Thus Mr. Curry, one of the physicians of Guy's hospital, remarks, that he had never escaped an epidemic catarrh but once, and then he laboured under a severe ophthalmia: and this was in the memorable influenza of 1782, the most general, perhaps, that has ever been recorded, (having traversed the earth in a westerly direction from the Tartar hordes, contiguous to the great wall of China, and finally spent itself in the West Indies and in this country.) Sir James Fellowes, who was in Paris during the reign of this epidemic, which was there termed '*la grisse*,' remarks that a similar coincidence, between it and ophthalmia, was likewise remarked there; with this difference, however, that in Paris the catarrhal pneumonia *preceded* the ophthalmia, but that the immunity was the same in both places; no person who had laboured under *la grisse* being known to suffer afterwards from inflammation of the eyes, and vice versa. This statement is confirmed by the further testimony of Dr. McDonnell of London, who was in Paris at the same time.

M. Demours makes mention of a similar epidemic catarrhal ophthalmia, which raged in Paris in the month of January 1806. He remarks, "*la conjunctive, n'etoit pas la seule membrane, sur laquelle portât l'influence de l'epidemie catarrhale qui regnoit alors. La muqueuse des gros intestins etoit affectée chez plu-*

sieurs des personnes qui me consultèrent—quelques-unes éprouvoient des accidens dyssentériques, qui opéroient promptement la solution de l'ophthalmie: mais c' étoit surtout la membrane muqueuse des voies de la respiration qui étoit le siège de l'épidémie."*

A more severe epidemic of this kind is related in the *Journal General de Médecine, de Chirurgie, et de Pharmacie*, Octobre 1811, which occurred at Vicenza in the year 1808.

We proceed to the second division of Conjunctivitis—or the *Conjunctivitis scrofulosa*.

It is scarcely necessary to remark, that this species of ophthalmia affects those only of a scrofulous habit. Hence it attacks more frequently children and young persons than adults, or such as are more advanced in age. I think I have most commonly seen it in children from the first to the sixth year. Very often it is the first symptom we have of scrofula in the constitution: more commonly, however, it is connected with the other characteristic marks of the disease, as thick upper lips, widely-expanded nostrils, swollen abdomen, a loose spongy texture of the fibre throughout the body.

The following are its chief characteristics. The eye-lids, particularly the upper, are much swollen and red, generally half-closed, more especially if the eye be at all directed towards the light. The patient can no longer move them; and any effort of the surgeon, made to separate them, causes the muscles of the face to be thrown into violent action. The orbicularis palpebrarum appears to be spasmodically contracted over the globe of the eye, which is drawn upward and outwards, so that its fore-part is mostly concealed behind the upper eye-lid. If the patient be an adult, and can exert sufficient resolution to open his eyes, it will be observed that he depresses at the same time his lower jaw and the angles of his mouth. He seeks out the darkest recesses of his room, and is never easy but in the most total darkness. If a child, he buries his eyes in the pillow of his bed, or in the lap

* Demours, sur les Maladies des Yeux, &c. Vol. I. p. 112. Paris, 1818.

of his mother, to screen them from the ordinary light of the day. Mr. Travers* remarks, "that he has known this intolerance to light, in more than one instance, of such intensity and duration, as to occasion a distortion of the spine from the habitual depression of the head, and the obstinate maintenance of an awkward and unnatural position of the body to screen the eyes from the light."

A quantity of acrid, pungent tears, are constantly distilling from the half-separated eye-lids, which, falling over the cheek, produce an uneasy sensation of scalding and itching, and cause the patient to be continually rubbing his eyes. Sometimes the lachrymal sac and canals are also affected, and a similar discharge takes place from the nares, excoriating the lip and alæ of the nose.

Some little address is required in opening the eyes of the patient in order to examine them. Children, in general, oppose most obstinately every effort of the surgeon, made with this view; and it is in vain to offer mild persuasion, or hold out any reward to gain their compliance. The best mode of proceeding is to place the head of the child between the knees of the surgeon, who is seated. Carrying the fore-finger of his right hand over the orbit, the skin of the superior palpebra should be embraced with its point, and gently raised. This should be managed very skilfully, or the tarsus will be everted. The lower lid can be depressed in the same manner, with the fore-finger of the other hand. It is never necessary to separate them far asunder, as the rotatory or convulsive motion of the eye will alternately expose every part of its surface. On examining the eye itself, we shall perceive throughout the conjunctiva a diffused and unequal redness; its vessels running in distinct fasciculi or bundles, as so many radii of a circle, towards the cornea. The most of these terminate on the circumference of this latter tunic—a few continue even to its centre—and others do not go beyond the sclerotic. These become more and more developed in the progress of the disease, and at the end of each there appears a phlycte-

* Travers, *Synopsis of the Diseases of the Eye*, p. 92. London, 1820.

nula, or small pustule, which, bursting, pours forth its contents, and changes into an open ulcer. These phlyctenulæ are often in great numbers upon the eye. Where they are seated upon the cornea, they gradually enlarge in size—corrode deeply into the substance of the tunic, or even pierce the anterior chamber. In this manner the chamber is filled with matter, and a new disease, *hypopion*, is superadded to the original inflammation. At other times, the aqueous humour, escaping through this fistulous opening, the iris falls forward, adheres to the cornea, and gives rise to what is termed *staphyloma iridis*. A large cicatrix being left from such union, the vision is either much impaired, or completely destroyed. If the ulcer have not penetrated entirely through the cornea, but merely through its outer lamella, the matter sinks into the loose cellular texture which connects this with the inner, and we have produced an *onyx*.

In a still higher grade of the disease, the vessels of the conjunctiva overshoot the margin of the cornea, and advance in radii to its very centre; the cornea, or rather the conjunctiva, which covers it, is seen of a reddish-brown colour, swollen and thickened, presenting more the appearance of a muscular or fleshy texture, than that of a membrane. The iris and pupil are entirely concealed, and the vision is completely obscured. Some authors suppose, that the cornea is really inflamed under these circumstances; hence they have given to this appearance the name of *Corneitis*. Professor Beer, who looks upon it as a mere thickening of the conjunctiva, terms it, with much more propriety, *Pannus*.

One of the peculiar characteristics of strumous ophthalmia, is, that it is entirely unaccompanied with pain. The same fact is observed with respect to scrofulous inflammation, when seated in other parts of the body. It is every where marked by a dulness of sensibility, and the pain, in comparison with that attendant upon other varieties of inflammation, is always very slight.

Strumous ophthalmia is moreover characterized, as we have already noticed, by great intolerance of light. This, to be sure, is a common symptom in all inflammations of the eye; but in this species, it is constantly the most predominant. Some endeavour

to account for this symptom, by supposing the retina to be affected; but if this were the case, we should, in all likelihood, find the vision much impaired after the removal of the inflammation, which seldom or never happens. Mr. Travers is disposed to consider this morbid irritability of the retina as dependent upon the state of the alimentary canal, with which it sympathizes, and this opinion will not appear altogether without foundation, when we reflect how often this affection is accompanied with derangement in the *primæ viæ*, as worms, sordes, &c.

“This state of intolerance bears an analogy to cases of depraved, or rather of painfully acute sensibility, in other organs of sense. The senses of hearing and of smell are, in some rare cases, rendered morbidly acute, independent of the slightest organic affection, so that the ordinarily agreeable stimuli of these organs in a state of health, viz. an acute sound and a pungent odour, become causes of distress. An amateur of music, when labouring under an occasional disorder of the auditory passages, compared his sensations during a fine performance of instrumental music, to those of Hogarth’s enraged musician—all was jar and discord. Every snuff taker knows the effect of a catarrh to spoil his enjoyment.”*

This intolerance of light is generally a source of much anxiety and alarm to the parents, who regard it as a most dangerous symptom; but I have seen it continue for months without any evil consequence: and, as long as the cornea continues clear and transparent, no danger is to be apprehended.

A third form in which inflammation may attack the conjunctiva, is that commonly known under the name of *purulent ophthalmia*.

Mr. Ware, I believe, has been the first to distinguish this species from the other varieties of ophthalmia: and from the circumstance of its being constantly attended with a puriform discharge from the lids, he called it the *oculus purulentus*. Until it shall be more satisfactorily determined that this secretion is really purulent, we would prefer the terms blepharo and ophthalmoblenor-

* Travers, op. cit. p. 93.

rhœa, given it by Professor Schmidt;* thus identifying its nature with the same inflammation which occurs in the other mucous structures of the body: and, in tracing its history and symptoms, we shall find the analogy perfectly just and consistent. Thus, blepharo-blenorrhœa, like blenorrhœa of the urethra, or of the mucous membrane lining the urethra, may be divided into three distinct stages, each possessed of its peculiar character, and following in like succession.

In giving the history and progress of the symptoms, I shall confine myself to the disease as it appears in infants; not only because these are the most frequent subjects of its attacks, but because in them its characters are more strictly defined, and better developed.

Sometimes this disease appears immediately at birth. More frequently, it commences six, eight, or fourteen days after birth. At other times, it is not seen until in the second, third, and sixth month. Mr. Travers† remarks, "that for one person affected with this disease above three months old, I should think that at least twenty are subject to it under that age." I have rarely known it to attack children in the second or third year. The first stage of the disease is characterized by a slight redness of the palpebral conjunctiva, accompanied with a small discharge of a mucous or puriform secretion from the lids. The eyes are impatient of light, and the child, on this account, is observed to keep them half closed, or opens them only towards evening, or when shut up in a dark chamber. The conjunctiva, covering the globe itself, is never red or inflamed in this stage of the complaint, the inflammation and swelling of this membrane being entirely confined to that portion of it which lines the palpebræ; these are always greater near the tarsus, gradually declining as the conjunctiva approaches the bulb. If the patient be more advanced in years, he will complain of a constant itching in the inner canthus, or lids, to allay which, he will be frequently

* Vid. Ophthalmologische Bibliothek. Band III. St. 2. p. 107. Other synonyms are, *ophthalmia neanotorum*, *ophthalmia puriformis*, *ophthalmia orphana tropheorum*, &c. &c.

† Op. cit. p. 97.

seen to rub his eyes with the finger; or he has the sensation of a grain of sand, or other extraneous body, rolling within the eye, similar to that we described when speaking of the catarrhal ophthalmia. This sensation, however, is not constant, but comes on suddenly, and as suddenly again departs. These symptoms becoming in a few days more violent, give rise to what may be termed the *second* stage of the complaint. The difficulty of separating the lids, or opening the eye; the intolerance of light; the redness and swelling of the conjunctiva; the mucous discharge from the lids, are all considerably increased. The discharge, which was at first whitish, thin, and inconsiderable, assumes, in the progress of the disease, a more viscid consistency, and is of a yellow or greenish colour, very much resembling genuine pus. If the surgeon in this stage separate the lids, in order to examine the eye, this matter will be discharged in such quantity, as to overflow of a sudden the lids and cheek; and if the disease be very violent, there will be mixed with the mucus flakes of a dark brown colour, consisting of exudated blood. In the still higher grades of the complaint, this blood will be found intimately united with the other secretions, so that a thin sanious discharge is constantly distilling from the eye. The quantity of this discharge is always proportionate to the intensity and extent of the disease. The less the discharge, therefore, the more mild the disease, and vice versa. It is always less in the commencement, gradually increasing until the second stage has reached its acme, when it again abates.

If the eyes be a little cleansed, by injecting between the lids a mixture of milk and water, or a mild solution of alum, the conjunctiva, particularly that portion of it which covers the superior palpebra, will appear much reddened and swollen. This appearance of the vessels of the conjunctiva has been compared by Mr. Saunders, and not unaptly, to the villous surface of the foetal stomach when finely injected. If examined in this state with a good microscope, the matter may be seen very distinctly oozing from the vessels which terminate upon its surface.

This swelling of the conjunctiva increases to such an extent in the progress of the disease, that the patient is in the end unable

to raise the upper eye-lid; and from the swelling, or from the matter collected beneath, it is so protruded from the eye-ball as to conceal, or cover entirely, the lower palpebra. Great care must be taken by the surgeon, in examining the eye under these circumstances, how he separates the lids; for if this be done forcibly, or during the struggles or crying of the child, the upper lid is very liable to be everted: and it is often difficult, nay, even impossible, to reduce it. It is on this account, that many surgeons have advised this examination to be made during sleep only.

As the swelling of the conjunctiva increases, the external surface of the lid is more and more reddened and swollen; and these appearances are observed, to a considerable extent, over the cheeks and face. As the patient cries, or struggles, this redness is seen to assume a more livid or purple colour. "Although the tumefaction of the palpebræ may be at first further advanced in one eye than in the other, it generally reaches its maximum of height in both about the same time. The patient, reduced to a state of great uneasiness by the irritation of the swelling, and by its confining the discharge, begins now to suffer attacks of excruciating pain in the eye itself.* This pain is sometimes periodical, coming on generally about bed-time, or in the course of the night. It is commonly confined to the globe itself; in other instances, it extends to the forehead, temples, or deep within the cranium, accompanied with the sensation, as if the eye were too large for its socket.

It frequently happens, before the puriform discharge is fully established, or in the commencement of the second stage, that some degree of hæmorrhagy takes place from the lids. This, instead of being an unfavourable occurrence, is always a desirable and pleasant circumstance, where the blood is of its natural, healthy colour, or consistency. The symptoms will generally be found to have considerably moderated, after such an occurrence. The swelling of the conjunctiva, when this has already taken place, is much diminished; and the subsequent discharge will be found not only lessened in quantity, but milder and healthier in

* Vetch's Practical Treatise on the Diseases of the Eye. London, 1820.

quality. When such an hæmorrhagy takes place, even in the more advanced stage of the disease, it never fails to be attended with considerable amelioration of all the symptoms.

If the inflammation, from the commencement, has spread rapidly from the conjunctiva of the palpebræ, to that portion which envelopes the bulb, the latter will be found not only reddened, but greatly swollen; presenting, as it were, a thick wall of fleshy substance around the cornea. This latter tunic appears, in consequence, as if sunk or imbedded in the eye, and very often little more is perceptible than its centre. If the matter be allowed to collect, or stagnate for any time upon its surface, it acquires a thick membranous-like consistency, which often leads the surgeon to believe that the cornea itself has entered into suppuration.

Where the disease is more violent, the conjunctiva covering the cornea becomes affected; the cornea itself is rendered so opaque, that the vision, if not completely destroyed, is at least very much impaired. If the disease in this state be neglected, or improperly treated, this tunic goes on to suppurate, and at length bursts. The pain is momentarily relieved by the rupture, and the patient flatters himself he is getting better. This, however, is altogether delusive, and the relief is owing merely to the escape of the humours, which before this occurrence had kept up an uneasy distention of the inflamed coats. Previous to such rupture of the cornea, numerous ulcers, penetrating more or less deep into this tunic, make their appearance, working their way by degrees entirely through, and giving rise to that species of staphyloma which, from its peculiar hue and granulated appearance, has obtained the name of mulberry staphyloma, or *staphyloma racemosum*. The cornea now either cicatrizes, losing altogether its transparency and convex form, or the lens and vitreous humour are protruded, and the whole eye enters into the state of suppuration.

It is seldom, however, that the disease advances to this violent grade, or produces such disorganization in the structure of the organ. More commonly, the cornea is not at all, or only slightly affected; and the purulent discharge from the lids, the redness and swelling of the conjunctiva, and all the other symptoms, after continuing ten or fourteen days, or longer, gradually abate, and

give rise to what may be termed the *third stage* of the complaint. The patient is now again enabled to separate the lids; the viscid matter which, in the second stage, had firmly agglutinated the palpebræ, is formed now only during sleep, and is easily washed away with a little milk and water; the conjunctiva is no longer so much reddened and swollen. In a few weeks the disease totally disappears.

The description we have here given of the purulent eye, applies with so much strictness to what I have to say of the Egyptian ophthalmia, that I scarcely deem it necessary to draw any distinction between the two diseases. It is hardly a question with the best informed army surgeons of the present day, whether the Egyptian ophthalmia was known in England, and upon the Continent, prior to the expedition of Egypt, or not. The fact is sufficiently established; and we are warranted in believing that this species of ophthalmia existed, not only among the troops, who produced it by the application of corrosives and other substances to the eyes, for the purpose of invaliding themselves, but likewise sporadically, and even endemically, in many schools and public institutions of Britain, long before the expedition, which was at first supposed to have originated the disease, was contemplated. There is one circumstance, however, in the case of the Egyptian ophthalmia, which considerably militates against our supposing it one and the same disease with the purulent eye, and would rather lead us to consider the former as an affection *sui generis*; and that is, its being contagious. Although this opinion is supported by many of the most eminent army and navy surgeons of England and the Continent, and is sanctioned by such gentlemen as Brigges, M'Grigor, Ware, Royston, Vetch, Farrel; and among the Italians, by Vasani, Omodeus, and more lately, by Professor Rust of Berlin; there are not wanting others, of no less authority, who assert the contrary, and attempt to prove that the disease is, under no circumstances, infectious. Such are Desgenettes, Larrey, Saveresi, Assalini, Lewis Frank, Weinhold, &c. From such a difference in the opinion of gentlemen of equal authority, it is impossible to draw any fair or just conclusion; and

the question must rest as we found it. All of them agree in asserting, that the disease is seated in the conjunctiva; but what the specific nature of the inflammation may be, which distinguishes it from the blenorrhœa we have just described, is by no means satisfactorily made out.

The purulent ophthalmia is most generally found to attack persons of a weak cachectic constitution of body. In this respect it differs from the Egyptian ophthalmia, which is most common in those of a robust and full habit. Children are more liable to it than adults; and those of parents in the lower or middle classes of life, who are badly fed and clothed, than the children of the rich, or of those who are well nourished. The disease is very common as an endemic, in lying-in or foundling institutions; which is no doubt to be ascribed to the noxious air generated from the confinement of so many individuals in one room.* It is a common opinion with many physicians, that the disease in infants is owing to the contact of the eyes with some acrid discharge from the vagina of the mother, in the passage of the child, or during birth. I am rather disposed to look for the exciting cause of the disease in the sudden change of condition of the child from the womb to the world. In the womb, the eyes are closed, and subjected at all times to an equal temperature. It is little wonder, then, that an organ of such delicacy, on being exposed to the glaring light of a fire or candle, as is too often practised by careless midwives and nurses, or to great vicissitudes of temperature, to currents of cold air, should suffer so severe consequences.

Assalini† and Larrey‡ both agree, that the Egyptian ophthalmia is not to be attributed, as many writers have supposed, to any peculiarity in the soil of that country, but rather to the common causes which excite ophthalmia in other latitudes: and the

* Messrs. Mongenot and Jadelot have given the history of two epidemics of this kind, that prevailed in the Hospital for Sick Children at Paris; which may be found in the Medico-Chirurgical Annuary for that city. Similar epidemics have been very frequent in Christ's Hospital, London.

† *Manuale di Chirurgia del Cavalliera Assalini*, parte II. p. 111. Milano, 1812.

‡ *Memoires de Chirurgie Militaire et Campagnes*, de D. I. Larrey, tome I. p. 208. Paris.

greater degree of violence, or the greater frequency of the complaint in Egypt, is accounted for by the more violent and frequent action of these causes. Thus, as in the case of infants immediately after birth, they refer the disease, and not without great reason, to the very vivid light and heat reflected from a sandy soil in these burning climates; or to the cold damp nights which succeed, in certain seasons of the year, to an intensely hot and dry day. It is nothing more, in their opinion, than a simple inflammation of a mucous texture, caused by *suppressed perspiration*. The same inflammation, attacking the lining membrane of the intestinal canal, produces the diarrhœas and dysenteries, so frequent and fatal in this climate: and the only reason of its falling more frequently upon the eye, is, that this organ, in a state of indirect debility, (to borrow an expression from the system of Brown), from the undue action of the light and heat through the day, is more predisposed to its attacks than any of the other textures or organs of the body.

X Assalini remarks: "Among the causes of ophthalmia, I consider the great light of the sun, as that which contributes most to excite in the delicate parts of the globe of the eye a considerable degree of irritation. It is not in Egypt only, that we observe complicated diseases of the eyes. At Bologna, in Italy, for example, a stranger is struck with the number of blind people who sing, or play upon different instruments, through the streets of this great city; and many individuals are to be seen there with the globe of the eye projecting, and more enlarged than in a natural state, with staphyloma, opacities, &c. It is well known, that these affections are the result of different inflammations, produced by the brightness of light, reflected from the walls of the houses whitened with lime. In Egypt, the view of immense arid plains, the repercussion of the rays of the sun reflected by the soil, united with the nature of the roads and places on the sandy borders of the Nile, of which the heat and splendour were so great in full day, that scarcely could a place be fixed on where one could wish to place his feet,—necessarily weakened, fatigued, and disposed the eye to particular affections." That the ophthalmia does not proceed from any of these sources, but that they merely give the predisposition, the author goes on to prove by a variety of facts;

and concludes with saying, "It seems to me more just to attribute this malady to the suppression of perspiration, which occurs very often in Egypt, principally during the night; and which, throwing itself upon the part most weakened, affects sometimes the intestines, but oftener the eyes, fatigued by the too vivid light of the sun. This, in my opinion, is the true source of the ophthalmia of Egypt. As for me, I have neither used spectacles, nor collyrea, nor any preventive to guard myself from the burning sand, the nitric, the ammoniacal, or calcareous dust, nor even against too great light; but, on the other hand, neither the freshness of the evening, nor the beauty of the night, has induced me to open my windows, or lie in the open air; and, when obliged to bivouac, my mantle served me as a tent, and became my ægis."*

The baron Larrey, in a letter on this subject to Professor Græfe of Berlin, inserted in the *Journal der Chirurgie u. Augenheilkunde* of that city, remarks, in corroboration of the opinion of Assalini, "that all such as covered themselves warmly through the night, and thus protected themselves from the sudden vicissitudes of the weather, escaped the disease." Volney, Denon, and numerous other travellers through Egypt, all agree in ascribing the disease to the humidity and coldness of the night air. The latter gentleman observes, "The heat of the days and coolness of the nights, in this season, had afflicted the army with much inflammation of the eyes. This is unavoidable, when long marches and fatiguing days are followed by night-watching, in which the humidity of the air represses perspiration—vicissitudes which bring on, either inflammation in the eyes, or in the bowels."

We may here take notice of another variety of this ophthalmia—the *Ophthalmia gonorrhœica*.

Many surgeons, of much eminence, have indeed, denied that gonorrhœa ever attacks the eye.† I have never seen the disease

* Vid. op. cit.

† Among the number of these, I may notice Mr. Pearson, who remarks, "that during a pretty extensive experience of twenty-five years, he has never seen a single instance of inflammation of the eyes, which was evidently derived from a gonorrhœa; and that of the many thousand cases of gonorrhœa which had fallen under his notice, he never could, in any one instance, trace such a connexion between the eye and urethra, as has been asserted by many eminent writers." Vid. note by Briggs to the last edition of Scarpa.

myself. It is one of very rare occurrence; but from the cases related by Swediaur, Beer, Scarpa, Vetch, and others, I have no doubt at all of its existence. It differs, perhaps, in no material point, from the ophthalmia we have just considered, excepting that its symptoms in general are much more violent, and its progress more rapid. Thus, the swelling of the conjunctiva, particularly that covering the globe, the pain of the eye, the intolerance of light, the purulent discharge over the cheeks, are all much more vehement than in the purulent eye, produced from other causes. All the authors above-named, with the exception of Mr. Vetch, agree, in supposing that the disease is generally induced by the sudden suppression of the gonorrhœal discharge from the urethra; arising from the use of too strong stimulant or astringent injections, from violent exercise, from the bathing of the parts in cold water, from bandages or compresses applied to the penis to prevent the discharge, from exposure to cold, after the body has been preternaturally heated, or from damp and cold feet, &c. Scarpa, it is true, in the last edition of his work, retracts the opinion mentioned in the former, "considering the metastasis of the gonorrhœa to the eye, as a mere chimera;" but Mr. Vetch has gone still further, and denies that the disease is ever communicated to the eyes from the gonorrhœal discharge of the same person. As decisive proof of this, he adduces the case of an hospital assistant, who, with more faith than prudence, conveyed the matter of a gonorrhœa to his eyes, without any affection of the conjunctiva.

"From this time," Mr. V. observes, "I was led to look for an explanation of the connexion subsisting between gonorrhœa and ophthalmia, arising in the same person, in some peculiarity of the constitution, and to conclude that the disease is an extension of an inflammation which first showed itself in the urethra, and of which the different structures of the eye are liable to participate in common with many other parts."*

* *Op. cit.* p. 243. M. Callerier, physician to the Hospice de Veneriens of Paris, mentions that he has seen the gonorrhœal discharge take place from the nares, and meatus auditorius. Dr. Duncan also relates an instance of the first kind, which was caused by the patient inadvertently blowing

That inflammation affecting a particular tissue in one part of the body may be extended to a structure of a similar kind in another, is less extraordinary, than that it should be propagated to one of a dissimilar character: and until the peculiar relation and laws which govern these different tissues in a state of disease, are more fully developed, the matter must ever remain inexplicable. Every practitioner, of any experience, must have remarked the various sympathies of these tissues; and it would be useless to multiply examples in proof. Thus, it is not unusual to find rheumatism, or inflammation of the synovial membrane of a joint, alternating with gonorrhœa, or affecting simultaneously the same individual. Gout is known to attack the mucous glands of the urethra, or those of the eye-lids.* It is more remarkable to find gonorrhœa, rheumatism, and ophthalmia, combined in the same person, yet instances of this kind are occasionally met with. Mr. Astley Cooper, in his lectures, mentions the case of an American gentleman, whom he treated for gonorrhœa, who had, at the same time, a severe ophthalmia, accompanied with rheumatic affection of both knees. He remarks that, at the time he saw this case, he considered the rheumatism and ophthalmia as a mere accidental circumstance; but, from having noticed similar cases since, he is convinced they have some real connexion with the gonorrhœa, and are often a consequence of it. Mr. Abernethy conceives this variety of the disease to be caused by too great irritability of the system, but more particularly of the mucous membranes, depending in general upon some deranged state of the digestive organs; hence, he terms it the *irritable* ophthalmia from clap. The following case he relates in his lectures, as corroborative of the correctness of his views and plan of treatment. "A gentleman, a seafaring man of robust and strong habit, but of very irritable and fidgety constitution, applied to him with a gonorrhœa. He was to have left town a few days after, to join his squadron at Plymouth, but was detained in London by a vio-

his nose, with the same handkerchief which he had employed to cleanse the penis. In some manuscript notes from Professor Thompson's lectures, I find a case related, in which the gonorrhœal discharge occurred from the rectum.

* Vid. Stoll, Barthez, J. P. Frank, Barton, Beer, &c.

lent ophthalmia, which immediately supervened upon the affection of his urethra. Mr. A. advised him simply to bathe his eyes in a little poppy water, and keep his bowels gently open; recommended a light and moderate diet, with the blue pill. In three days after he had commenced upon this system, his eyes were considerably amended, and much less irritable to the light; but he now complained of a severe rheumatic pain of the shoulder. In two days more the pain attacked both knees, and there was a sensation of effused fluid in the joints. A few days after, when Mr. A. visited him, he found him sitting up in his chamber, with his windows thrown open, and completely relieved of all pain in the joints and eye, and with nothing remaining but the discharge from the urethra. Copious dark and bilious stools had come on in the course of the night; the biliary secretions were now corrected, and the patient was entirely cured. Mr. A. remarks that he has seen a great number of these cases, which have all yielded in a very short time to the same mild plan of treatment: in other instances, where he has known an opposite system pursued, where the patient has been plentifully bled and purged and blistered, he has seldom failed to have incurable opacities remain behind." Whether this species of ophthalmia, noticed by Messrs. Cooper and Abernethy as a frequent concomitant of gonorrhœa, be of the purulent or rheumatic kind; in other words, whether the inflammation was seated in the conjunctiva or sclerotica, we are at a loss to determine, as the symptoms in neither case are very strictly defined. No discharge from the lids is alluded to; and no more is said, than that the patients were attacked with severe pain and redness of the eye, and great intolerance of light; symptoms equally characteristic of both inflammations, and from which we can draw no positive conclusion. From the circumstance of its alternating with rheumatism, we are inclined to suppose it to have been rheumatic ophthalmia of the sclerotic coat; and we are warranted in this supposition from some remarks of Mr. Vetch, on a similar disease which occurred to this gentleman. He says, "the inflammation of the eye appeared to have had its seat in the sclerotic coat; and on examining it more closely, I found an irregular and contracted pupil, with some

opacity of the capsule of the lens, and adhesion between it and the iris: and on causing him to shut the sound eye, the vision of the left was found to be very much impaired. In neither eye was there any symptom of purulency or chemosis to indicate disease of the conjunctiva."*

Professor Beer has noticed several other species of Conjunctivitis, as the morbillous, scarlatinous, erysipelalous, &c.; but, as these are of extremely rare occurrence, I shall pass them by for the present, without description.

[We have to regret the obligation which we feel to give publicity to the following communication. We are anxious to avoid every thing which may have even the appearance of a wanton or unnecessarily severe attack upon any individual. Yet if the charges adduced against the gentleman referred to in this communication be *true*, we know not how to justify ourselves in refusing it a place in the Recorder. It comes from a highly respectable quarter; and certainly recommends itself by, at least, the semblance of justice which it wears, and by the ability with which it is written.

EDITORS.]

ART. IX. *Remarks on Certain Parts of "An Account of the Yellow or Malignant Fever, as it occurred in the City of Philadelphia, in 1820. By SAMUEL JACKSON, M. D. President of the Board of Health, and one of the Vice-Presidents of the Philadelphia Academy of Medicine."* Philadelphia, 1821.

IN a very unceremonious and uncourteous attack made upon the author of "A Discourse on Medical Police," by Dr. Jackson, and published as a kind of Appendix to his Account of the Yellow Fever, I find so many errors and misrepresentations, that, as a friend to common justice I cannot suffer it to pass unnoticed. If, in the examination of this subject, it shall be found unavoidable to make such an exposure as will redound but little to the credit of Dr. Jackson, I hope it may not be considered an act of unnecessary severity. A public censor who is detected in the

* Op. cit. p. 245.

very practices against which he is inveighing with unsparing bitterness, is certainly entitled to little mercy and less pity. If he sins from ignorance, he may indeed be pitied for his weakness, at the same time that justice should not be defrauded of her right by suffering the offender to go unpunished.

Under these impressions, I shall proceed to notice the principal charge which Dr. Jackson urges against Dr. Hosack; which is, that throughout his work on "*Medical Police*," he makes "*a deceptive reference to authors for opinions, on subjects of which they do not treat, and for facts they do not mention.*" (Jackson on Malignant Fever, &c. p. 101.) I perfectly coincide in opinion with Dr. Jackson, that "this is a serious allegation, and should not be lightly made." Having hazarded so "serious an allegation," however, it is to be presumed that he offers solid and unanswerable facts and arguments in support of its truth. Let us see whether he justifies this reasonable expectation. His first specification under the general charge is as follows: "The names of Huxham, Haygarth, Currie, Gregory, Ferriar, Percival, Blane, Chisholm, M'Gregor, Pym, Gilpin, and Wright, are quoted at pages 8 and 9, in a manner to lead those who are not conversant with their writings to believe, that they have all been familiar with yellow fever, treat of it in their works, and inculcate its contagion. But what is the fact? Huxham, Haygarth, Currie, Gregory, Ferriar, Percival, and Wright, never saw yellow fever: and their writings, with the exception of a feeble paper by Dr. Haygarth, are confined to diseases observed in England," &c. p. 101.

To show how complete a misrepresentation this is of the language of the Discourse on Medical Police, it will only be necessary to quote the passages referred to. After speaking of the opposition made by some of the medical gentlemen of this country, to the doctrine of the contagiousness of yellow fever, the author adds, "They have precipitately run into the absolute and unqualified disbelief, not only of the correctness of the view taken of this subject, but of the contagiousness of the fevers enumerated, under any circumstances. They have, indeed, considered the very existence of contagion, as appertaining to fevers, a mere phantom of

the imagination. To these gentlemen I observe, in the language of an able writer in the *Edinburgh Review*, that 'in the present state of medical knowledge, it is not at all more absurd to deny the existence of fever altogether, than to maintain that it is not propagated by contagion.' I furthermore ask, are the unfledged opinions and speculations of those men of the closet, who yet have had but few opportunities to test them at the bed side, to come in competition with the results of experience, aided by the most profound professional learning, and the distinguished abilities of the numerous writers who have adorned the past, and many of whom still continue to enlighten the present age? Are the investigations and the accumulated experience of Huxham, Haygarth, Currie, Gregory, Ferriar, Percival, Blane, Chisholm, M'Gregor, Pym, Gilpin, Wright, and a host of others, to be prostrated, &c. &c." *Discourse on Medical Police*, p. 8, 9.

It is impossible to conceive by what profound process of critical legerdemain, Dr. Jackson extorts from this plain language a meaning so opposite to that which it is intended to convey? Dr. Hosack does not assert, even by implication, that any of the writers referred to had seen yellow fever, or treated of it in their writings. He is not speaking of yellow fever, but of "contagion as appertaining to fevers, in general," and it is in this connexion, and in support of this doctrine, that he brings forward the names of these distinguished men. And, in doing this, I am confident that no man acquainted with their writings will pretend that the least injustice has been done them. They all, without an exception, admit contagion to be an attribute of fevers.

Having accused Dr. Hosack of referring, in support of his opinions concerning yellow fever, to authors who had never seen that disease, nor treated of it in their writings, Dr. Jackson triumphantly asks, "But what is the fact? Huxham, Haygarth, Currie, Gregory, Ferriar, Percival, and WRIGHT, *never saw yellow fever*: and their writings, with the exception of a feeble paper by Dr. Haygarth, are confined to diseases observed in England," &c. It happens rather unfortunately for Dr. Jackson's reputation, that he should express himself in a tone so peremptory on subjects concerning which he really seems to know so little. He

asserts positively here that Dr. Wright never saw yellow fever, and that his writings are confined to diseases observed in England. Now, Dr. Jackson must either be ignorant of the Dr. Wright referred to, or he must have designedly confounded him with another person of the same name, who wrote an account of the Walcheren fever; for the Dr. Wright mentioned in the Discourse on Medical Police lived in the West Indies, saw yellow fever, and wrote upon it. Willing to put the most favourable construction upon Dr. Jackson's conduct, I shall attribute it to his ignorance; and, for his information, shall take the liberty of telling him, that the gentleman mentioned by Dr. Hosack is Dr. William Wright of Jamaica, a very distinguished physician, and known to every student of medicine who reads his Dispensatory, for his description of the *Cinchona Caribæa*, and the *Geoffræa inermis*. He is a Fellow of the Royal College of Physicians of Edinburgh, and of the Royal Societies of London and Edinburgh, and was at one time physician to the forces in the West Indies. Several of his papers may be found in the Transactions of the Royal Society of London, notices of which may be seen in Duncan's Medical Commentaries. In addition to his other productions, an essay of his was published in the seventh volume of the "Medical Facts and Observations," of London, entitled, "Practical Observations on the Treatment of Acute Diseases; particularly those of the West Indies," in which he treats especially of yellow fever. After this developement, what are we to think of Doctor Jackson's assertion, that Dr. Wright "never saw yellow fever," and that his "writings are confined to diseases observed in England!" But Dr. Wright furnishes still further testimony against Doctor Jackson. As one of the instances of "deceptive reference to authors for opinions on subjects of which they do not treat, and for facts they do not mention," with which Dr. Jackson charges Dr. Hosack, the name of Dr. Wright is conspicuously introduced. Let us see with what justice. I have already shown that Dr. Wright has written on the yellow fever. It is therefore not a fact, that he has "been referred to on a subject of which he does not treat." And to show that Dr. Jackson is profoundly ignorant of Dr. Wright's opinions concerning contagion,

I will present him with a few extracts from the essay mentioned above. "Some late authors," says he, "who have written on West India diseases, have roundly asserted, that in tropical climates fevers are not contagious; but whoever has had the care of crowded hospitals, of jails, of ships of war, or of transports full of troops, must have seen numerous and fatal instances of contagion in the West Indies; more especially where cleanliness and free ventilation have been neglected."*

In speaking of the yellow fever, he expresses himself in the following terms: "Dr. Rush has classed this disorder with remittents; but every one who has practised in the West Indies, knows for certain, that the remitting fevers of warm countries are not contagious. There remains not a doubt in my mind of the yellow fever being typhous, exalted to a great degree of virulence from climate, situation, and other adventitious circumstances."†

And again, "All the letters I have had from my medical friends agree, that this fever (yellow fever) is highly *contagious*, and that new comers are most subject to receive it; particularly such as are young, or addicted to drinking spirituous liquors. *Next to these are the nurses and attendants on the sick, who breathe the air in their chambers, or handle their bodies or bed-clothes, &c.*"‡ From all this, it appears very evident that Dr. Jackson has been rather precipitate, in charging the author of the Discourse on Medical Police with making "a deceptive reference to Dr. Wright for opinions on subjects of which he does not treat, and for facts he does not mention."

Huxham is the next author adduced by Dr. Jackson, in proof of his accusation against Dr. Hosack for a "deceptive reference to authors." Hear his swelling language. "Had Dr. Hosack perused the volumes of Huxham, he would have found principles inculcated, derived from extensive observation, and drawn from the rich stores of medical learning, entirely foreign to those attributed to him, but which are the favourite themes of the Professor.

* Medical Facts and Observations, Vol. VII. p. 6.

† Ibid. p. 6, 7.

‡ Ibid. p. 8.

The contagious doctrine of Dr. Hosack, as applicable to epidemic diseases, it will be seen by the following extracts from the works of Dr. Huxham, is not countenanced by that high authority." p. 102.

As a specimen of one of these extracts take the following: "The depraved constitutions of the atmosphere are the causes of almost all epidemic diseases." Now, by turning to Huxham, it will be found that this is a garbled extract from a passage, which goes directly to support the opinions of Dr. Hosack. To enable the reader to judge of the disingenuousness of Dr. Jackson, I shall quote the whole paragraph. "The depraved constitutions of the atmosphere are the causes of almost all epidemic distempers; nay, even the increase and duration of such as are properly styled contagious, greatly depend on such constitutions. For, do we not see in one year, for instance, in this town, or that city, one or two only seized with the small-pox, or measles; whereas, in another year, from one person, at first seized, the contagion spreads to immense numbers; so that the very air seems to foment, or suppress the contagion, like as a spark of fire thrown on proper materials, bursts out into a vast conflagration; whereas, falling into water, or the like, it is presently extinguished."* It is impossible for words more precisely to express the views of Dr. Hosack, concerning the contagiousness of yellow fever. He has uniformly maintained that this disease is, or is not propagated by contagion, according to the state of the atmosphere into which it is introduced. It is, therefore, *not a fact* that sentiments have been attributed to Huxham by the author of the "*Discourse on Medical Police*," which are "not countenanced by that high authority." After pronouncing a merited eulogium upon Dr. Huxham, Dr. Jackson adds, "It is almost a species of profanity and sacrilege to abuse such an authority, by imposing opinions and doctrines on his works not to be found in a page of his writings." Of such profanity and sacrilege has Dr. Jackson been guilty, and that too in the very act of attempting to fasten that odious charge upon another individual!

* Huxham's Works, Vol. I. p. 2.

In the *second* specification, Dr. Jackson selects the following paragraph as the subject of his criticism. "Lind, Pringle, Blane, Lempriere, Neill, and Pym, have most abundantly drawn the distinguishing characters of bilious and yellow fever." p. 11. As nothing is said against Lind, Blane, Neil, and Pym, it is presumed that Dr. Jackson admits that they have been correctly referred to. Pringle and Lempriere are the only two against whom he objects. A remark or two on the latter, will exhibit a specimen of Dr. Jackson's usual mode of sophistical reasoning. He first admits the correctness of the reference to Lempriere, and then immediately shifts the ground of his remarks, by imputing to the Discourse on Medical Police sentiments which it never uttered. Let us hear his own words: "Lempriere," says he, "*it is true*, distinguishes between what he calls the continued endemic, and endemic remittent." Why, then, accuse Dr. Hosack of a "deceptious reference" to this author? "But," continues Dr. Jackson, "he positively denies that either is contagious." Who ever asserted the contrary! certainly the Discourse on Medical Police does not. The only point on which Lempriere is referred to is, that he has drawn the distinguishing characters between bilious and yellow fever. Not one word is said about his opinions on the subject of contagion. The charge of "deceptious reference," therefore, falls to the ground like "the baseless fabric of a vision;" and Dr. Jackson is left in the unpleasant situation of having made a serious accusation against the veracity and honesty of another, which, according to his own confession, turns out to be untrue.

I come now to Dr. Jackson's *third* specification. "A similar inattention," says he, "to correctness, occurs at page 13, as will appear in the following extract. 'I must however state, that until the facts on this subject, (contagiousness of yellow fever), which have been adduced by Sir Gilbert Blane, in his well known work, his Diseases of Seamen, and the body of evidence contained in the celebrated production of his later years, his Elements of Medical Logic, shall be disproved, &c.' " Against all this Dr. Jackson most solemnly enters his protest. "It is in vain that we," says he, "refer to its pages for a *single fact*, or *observation*, to

show the 'contagiousness of yellow fever.' On the contrary, frequent mention is made of the malignant fever of the West Indies prevailing on board the fleet; but it is always attributed to exposures to marsh effluvia on shore, and allusion is no where made to its spreading among the crew by contagion," p. 104.

After the disclosures already made, we are prepared for any thing that may come from the President of the Philadelphia Board of Health. The reader will accordingly learn, without surprize, that the whole of this is nothing more than the creation of Dr. Jackson's heated imagination. Sir Gilbert Blane, in this very work, relates one of the strongest facts ever adduced in support of the contagiousness of yellow fever. It is contained in a letter addressed to the Hon. Rufus King, then minister at the court of St. James. "The first question that occurs," says Sir Gilbert, "with a view to preventive measures, is, whether this disease (yellow fever) be infectious, and under what circumstances it is so? In those situations in which I observed it in the West Indies, it was evidently so. There was the most incontestible evidence of this, both on board of ships and at hospitals; and the doubts which have been started on this point, seem to have arisen from the operation of infection being blended with that of other causes, which must concur with it, in order to give it effect. But whatever doubts there may be on this subject in the West Indies, there can be none in the climate of North America. This will be best proved and illustrated by an example. On the 16th of May, 1795, the Thetis and Hussar frigates captured two French armed ships, from Gaudaloupe, on the coast of America. One of these had the yellow fever on board, and out of fourteen men sent from the Hussar to take care of her, nine died of this fever before she reached Halifax on the 28th of the same month, and the five others were sent to the hospital, sick of the same distemper. Part of the prisoners were removed on board of the Hussar, and though care was taken to select those seemingly in perfect health, the disease spread rapidly in that ship; so that near one third of the whole crew was more or less affected by it. This fact carries a conviction of the reality of infection, as irresistible as volumes of argument; and it further affords matter of important and

instructive information, by proving, that the infection may be conveyed by the persons or clothes of men in health." Blane's *Diseases of Seamen*, p. 605—607, 3d edit.

Dr. Jackson's noisy invective against "*The Elements of Medical Logic*," proves nothing so strikingly as his own incompetency to deliver a correct and impartial judgment concerning the merits of that celebrated production. If I thought that the work could in the least possible manner be injured by any thing that Dr. J. has alleged against it, I should enter into an examination of his accusations; but as it is impossible that this should be the case, I will leave the work to speak for itself. It will remain a monument of the abilities of its author, long after the memory of Dr. Jackson's futile attempt to asperse it shall have passed away.

Drs. Chisholm, Pym, and Wright are next arraigned, and formally condemned by the Philadelphia President. All these writers were adduced, in the *Discourse on Medical Police*, as having furnished affirmative testimony in favour of the contagiousness of yellow fever. That they do furnish such testimony, no one who is acquainted with their writings will deny. Whether that testimony is sufficient to induce a belief in the doctrines which it is intended to uphold, is quite a different question. Dr. Jackson, instead of fairly meeting this testimony, and endeavouring to weaken or destroy its force by counter testimony, flies off from the subject matter of dispute, and bends his whole force against something that never was contended for. For example, instead of proving that Dr. Chisholm had furnished no affirmative testimony in favour of contagion, as he should have done to make good his charge of "deceptive reference," on the part of Dr. Hosack, he confines all his remarks to Dr. Chisholm's theory of the origin of yellow fever on board the *Hankey*; a point which even Dr. Jackson cannot but know, is quite distinct from that of the contagious character of yellow fever. Not a word is uttered in the *Discourse on Medical Police* about Dr. Chisholm's theory, or about the ship *Hankey*. This is not the first instance in which Dr. Jackson has mistaken one thing for another. He seems, not unfrequently, to be very much in the predicament of Don Quixote

when he mistook windmills for giants. Indeed, in more traits of character than one, do these two knights-errant seem to resemble each other. I will not trace the analogy between them too far, for fear it might offend Dr. Jackson; but I cannot refrain from observing, that, in point of rashness and precipitancy, it would be difficult to determine who should bear away the palm.

Dr. Jackson appears to labour under the same delusion concerning Mr. Pym. He says, "As that gentleman builds his system on the verity of Dr. Chisholm's account of the origin of what is absurdly called the Bulam fever, they fall together." p. 107. He then leaps at once to his conclusion: "Thus, the most material part of the affirmative testimony, on which Dr. Hosack lays so much stress, *has been completely disproved*; and that it stands in this predicament, ought to have been known to him." p. 107.

But, seriously, does Dr. Jackson expect to impose upon the public by reasoning such as this? Assertions, without proofs, and conclusions, without premises? If he does, he certainly could not have expected that his book should be read by the intelligent portion of the community. Mr. Pym does not build his system upon the origin of the yellow fever on board the ship Hankey. All this may be disproved, and yet Mr. Pym's doctrines remain unimpeached. If Dr. J. has read the work of Mr. Pym, (*and I take it for granted that he has read all the works he refers to, otherwise he would not dare to accuse another of deceptive reference to them,*) he must know that it contains a large body of "affirmative testimony," which has nothing to do with the story of the Hankey. As a specimen of this testimony, I shall quote one fact for the benefit of Dr. Jackson. Mr. Redmond, surgeon to the 54th, states the following, in a letter to Mr. Pym. "Upon the arrival of the 54th regiment in that island, (Jamaica) it was quartered at Stony Hill; a post, from its high situation, in general healthy, and from its detached position, had but little communication with other corps. In this quarter it remained nearly eighteen months, without any appearance of the disease, until a detachment was sent to Fort Augusta, and quartered, together with others, in barracks with the 2d West India regiment, where several men contracted the disease; and upon the detachment's return to the

hill, the fever passed progressively through the regiment. In a few days my two assistants, (one of whom died), and twenty out of twenty-one hospital attendants were infected." Pym, p. 72, 73.

Will Dr. Jackson pretend, that the credibility of a fact of this kind has any thing to do with the contested question of the origin of the fever on board the *Hankey*? and yet if there is any meaning in words, such is Dr. Jackson's argument. It is a perfect specimen of that kind of sophism which the logicians term *ignorantia elenchi*, or a mistake of the question.

Dr. Wright is the last author noticed by Dr. Jackson, under this specification. He says, "the Dr. Wright mentioned in this catalogue of names, has written no other work, that is known in this city, than a *History of the Walcheren Remittent*." p. 107. I have already shown that this is not the Dr. Wright referred to in the *Discourse on Medical Police*. Enough has been said to expose the utter ignorance of Dr. Jackson on this head. But there is another point on which I would beg leave to ask a single question. When speaking of Dr. Wright, two or three pages before this, he asserts that his "*writings are confined to diseases observed in England*." p. 101. Now, without pretending in the least to call in question his knowledge of geography, I would beg the Philadelphia President to inform me whether the "*Walcheren fever*" occurred in England? Dr. Jackson may think me impertinent; but he seems to possess such novel views, on a variety of subjects, that I am exceedingly anxious to become more intimately acquainted with them.

The *fourth* specification, under the charge of a "deceptive reference" to authors, on the part of the author of the *Discourse on Medical Police*, is, that Diemerbroek, Rondeletius, Clavigero, Herrera, Howard, &c. have been falsely referred to in support of the position "that animal matter will not generate pestilential fever." p. 108.

It is really diverting to see with what familiarity Dr. Jackson speaks of authors whose names are barely known to a majority of the medical profession, as if he were extensively versed in their writings, and thoroughly acquainted with their opinions. With

what modest confidence does he speak of Diemerbroek! "He no where adduces any facts which show, nor does he express his belief, that animal matter will not generate pestilential fevers."

It is truly astonishing how Dr. Jackson could have mustered up assurance enough to make such an assertion; for, although he may never have seen the works of Diemerbroek, yet in a paper which Dr. Jackson himself refers to, Dr. Ferriar quotes a passage from that author, which expresses, in language the most unequivocal and positive, his belief that animal matter will not produce pestilential disorders. To show in what manner this was viewed by that enlightened physician, I shall extract his remarks at some length. "It is a general opinion," says Dr. Ferriar, "that pestilential disorders are occasioned by the effluvia of dead bodies; but there is reason to question the truth of this. When plague has appeared, in the neighbourhood of places where many bodies had remained unburied, after general engagements, other causes can be pointed out, as more likely to have produced it. But many instances can be produced, in which thousands of dead bodies have been left to putrefy on the field of battle, without causing pestilential fevers. This was not unnoticed by the attentive Diemerbroek. 'Cadavera, sive hominum,' says he, 'sive aliorum animalium putrescentia pestem non generare, docent multæ magnæ strages, in quibus talis cadaverum inhumatorum putrefactio nullas pestes induxit. Anno 1642, in agro Juliacensi, maxima strages facta est, et ad minimum 8000 militum, occisa fuerunt, præter majorem adhuc famulorum, rusticorum, aurigarum, puerorum et mulierum numerum, atque equorum copiam innumerabilem; corpora inhumata sub diu computruerent *nulla tamen pestis insecuta est*. Hic in Germania, durantibus his nostri ævi crudelissimis bellis, etiam plurimæ maximæ strages factæ sunt, post multas, tamen illarum *nulla peste subsequente*." (p. 31.) These facts," continues Dr. Ferriar, "are strengthened by a well known circumstance, that in no case could the origin of a putrid fever be ever traced to the effluvia of dead bodies in a dissecting room. Nor have fevers been observed to originate, or to rage more severely, in houses surrounding church yards, in the middle

of large towns, though the stench of the putrid bodies, overheaped in such receptacles, is often insufferably offensive." Medical Histories and Reflections, vol. I. p. 274, 5, 6.

I shall leave Dr. Jackson to his own reflections, when he compares the foregoing with his assertion that Diemerbroek "*no where expresses his belief that animal matter will not generate pestilential fevers.*"

Rondeletius asserts that he had dissected bodies dead of the plague, in the presence of many of his pupils, with perfect safety. This fact was adduced in the Discourse on Medical Police in support of the general proposition that "animal matter will not generate pestilential fevers." Dr. Jackson, however, can see no possible connexion between the fact and the inference. He says, "it is impossible to perceive on what grounds Rondeletius has been adduced, as affording any testimony in this case." p. 109.

With a certain class of men, whose mental obliquity will not permit them to see the plainest things, it is useless to reason. I shall accordingly not undertake the fruitless task of attempting to convince Dr. Jackson, that the fact mentioned by Rondeletius bears directly upon the point in dispute; but shall content myself by confronting him with a physician of the highest fame, and whose powers of ratiocination were perhaps quite as good as Dr. Jackson's. Dr. Ferriar could see the importance and relevancy of the testimony of Rondeletius, although Dr. Jackson cannot. After relating the opinion of Diemerbroek, that animal matter will not generate pestilential fever, Dr. Ferriar adds, "*these facts are strengthened by a well known circumstance, that in no case could the origin of a putrid fever be ever traced to the effluvia of dead bodies in a dissecting room.*" Medical Histories and Reflections, vol. I. p. 276.

Clavigero is the next author who is arraigned by the President of the Philadelphia Board of Health. "I am unacquainted with any facts," says he, "contained in his history of Mexico, that can authorize the reference made to him." p. 101.

That Dr. Jackson should be unacquainted with the facts stated by Clavigero, can excite no surprise. He might have made the same confession long since concerning several other authors, of

whom he speaks with the most confident boldness. The following reference to Clavigero, I take from Dr. Chisholm, and is conclusive against Dr. Jackson. "Clavigero, on the authority of Torquemado, says, that at the dedication of the great temple of Mexico, anno 1486, 72,344 human beings, prisoners taken in war for the purpose, were sacrificed to the Mexican gods; and that a petty king, or lord, about the same time, in imitation of his master, the emperor, sacrificed many thousands on a similar occasion. On the erection of the great altar at Mexico, more than 12,000 were offered up; and the annual average of human creatures thus disposed of, amounted to 20,000, beside a prodigious number of quadrupeds and birds. Notwithstanding this dreadful waste of human blood; notwithstanding the horrible stench always present in this quarter of Mexico, the diseases, among an immense population, some say six millions in the city alone, were trifling, and proceeded almost altogether from marsh miasmata. The bodies of the victims were precipitated to the bottom of the steps of the altar, there to putrefy; or were sometimes ate by the Mexicans; and a pond of water, situated close to the great temple, was continually tinged by the blood of the sacrifices." See Hist. of Mexico, vol. I. p. 201, 232, 281, 426.

In his remarks concerning Mr. Howard, Dr. Jackson is equally unfortunate. "The observations of the philanthropic Howard," says he, "in his work on Prisons, will not bear the interpretation attributed to them. They may be considered as conclusively showing, that the contagion of typhus is not generated from filth; but certainly do not warrant the inference, that animal putrefaction will not occasion pestilential fever." p. 110.

The following quotation will abundantly expose Dr. Jackson's incorrectness. "It is remarkable," says Mr. Howard, "that when the corpse is cold of a person dead of the plague, it does not infect the air by any noxious exhalations. This is so much believed in Turkey, that the people there are not afraid to handle such corpses. The governor at the French hospital in Smyrna told me, that in the last dreadful plague there, his house was rendered almost intolerable by an offensive scent, (especially if he opened any of those windows which looked towards the great

burying ground, where numbers every day were left unburied;) but that it had no effect upon the health either of himself or his family. An opulent merchant in this city likewise told me, that he and his family had felt the same inconvenience, without any bad consequences." Howard on Lazarettos, 2d ed. Lond. p. 25.

I have now finished what I proposed in commencing this paper; which was to show, that instead of convicting the author of the Discourse on Medical Police of "a deceptive reference to authors," Dr. Jackson himself has been guilty of that crime. It will be observed, that in doing this I have confined myself to a simple statement of facts, without indulging in those harsh reflections which the occasion would seem to have rendered almost indispensable. I am free to confess, that I have not done this from any feeling of delicacy towards Dr. Jackson; for his offence against all the rules of literary courtesy has been of so flagrant a character, as to render lenity a crime. But I have conceived, that nothing that I could say in the form of censure or remonstrance, can be half so bitter as his own reflections; and to these I accordingly consign him. If Dr. Jackson's "bed be a bed of torture, he has made it for himself."

ART. X. *Remarks on the American Pharmacopœia, and on some late Reviews of the same.* By JACOB BIGELOW, M. D. of Boston.

To the Editors of the American Medical Recorder.

GENTLEMEN,

My attention has been lately drawn to two reviews, communicated and published in the July number of your journal, the design and tendency of which is to cast opprobrium on the result of the late efforts which have been made to establish in this country a National Pharmacopœia. It may appear unnecessary that an individual who has had but a subordinate and very limited part in the promotion of this work, should appear before the public

in its vindication. But as I am personally and repeatedly alluded to in these reviews, in a manner which the public can neither mistake, nor, I trust, approve; I am compelled, in justice to my own reputation, to remove the misrepresentations which the public have been called on to receive. I assume this task the more willingly, from a hope that the remarks I have to offer may exert an influence, in some degree beneficial, on the general subjects with which they are connected.

I presume there is no doubt that the medical profession are generally agreed with your first reviewer, when he states that "every enlightened practitioner of the United States must acknowledge that the establishment of a National Pharmacopœia, which should be so compiled as to secure the sanction of the different parts of the Union, is a desideratum of the first importance to the interests of the practice of medicine in this country." On the general usefulness of this design, there seems to be but one opinion; and it cannot be necessary to descant on the propriety of an undertaking, the utility of which has never, to my knowledge, been questioned.

But I must differ totally from your reviewer, who announces that an extraordinary degree of public expectation preceded the appearance of this work. On the contrary, never within the sphere of my observation was public expectation more moderate and distrustful. Never, in any similar work, was there less reason to expect the complete success of a first attempt. Persons conversant in medical literature, and acquainted with the history of previous pharmacopœias, well knew, that such works have uniformly arisen from imperfect beginnings; that they have approached to maturity by successive revisions, but have none of them arrived at it; and that a perfect pharmacopœia is a thing hitherto unknown. At the present day, the two most enlightened capitals of Europe, London and Paris, regulate their medical prescriptions by pharmacopœias allowedly, and, if we may believe the journalists of the day, notoriously defective. Yet these works have been made amidst a concentration of learned men, residing in the same city, enjoying the opportunities of daily conference, and of full experiment. They have been kept from

perfection by difficulties inseparable from works of this kind; by the unsettled state of chemistry and medicine; the discordance of individual sentiment in all matters of opinion; and the final necessity of compromises not always promotive of the views of any of the co-operating parties, but necessary to produce the authority and sanction, without which such works would be useless.

What, then, were the kind of expectations to be reasonably indulged in regard to an American pharmacopœia, to be formed, not by a single society, in a place of common residence, acting perhaps under the sanction of a government; but by a score of different societies, scattered over an extent of two thousand miles, called on by no mandate but the public good, and acting under a scheme which offered no reward for individual labour and sacrifice? The intelligent part of the medical public foresaw, that when a convention, under the delegated power of these societies, should have assembled at Washington, the great inconvenience and expense to which its members would be subjected, must render its session short; that it could not go into the details necessary for the completeness of the undertaking, and that the pharmacopœia which its members should frame, although it might be sufficient and useful, must unavoidably contain both oversights and defects. And, after this convention had framed their pharmacopœia, and directed its publication by a committee residing in five different cities, with intervals of an hundred miles, two of which committee were never present at the convention; it was evident that the difficulties of publication must be equal to those of compilation; that this committee, occupied at their own houses and about their own concerns, would probably never come to a full meeting; that in any partial meetings they could do little more than arrive at an understanding of the views and requisitions of the convention, and that the labour of writing out a copy for the press must be divided among different hands, none of whom had a right, in material questions, to contravene the doings of the convention, or of each other. So that the chance of discrepancy and imperfection must be increased, rather than diminished, by this distributive arrangement.

Nevertheless, a national pharmacopœia could not have been

formed in any other way. From the usages of our country, no medical body, but a representative one, could have given to such a work the authority to become a standard. The difficulty did not exist in the framing of a competent pharmacopœia. A practised individual, a well bred chemist or apothecary, could have done this sufficiently well. But the arduous part of the undertaking was to meet, and appease the various conflicting interests and sectional jealousies, which it was foreseen would be excited against its introduction. The convention, perhaps wisely, adopted measures for publication, which were calculated, as far as possible, to divest the work of any local character. This sacrifice to expediency, however, increased the difficulty of producing, in a first edition, the entire accuracy and correspondence of different parts which it is desirable ultimately to attain.

Considering that my own reputation, for reasons hereafter to be stated, has but little connexion with that of the pharmacopœia, I feel no backwardness in stating my belief, that the work which the convention have produced, is fully adequate and competent to supply the exigencies of the medical community; that it is better than could have been reasonably expected of a first specimen, considering the mode of its formation; that it has oversights, errors, and defects, yet of a nature capable of being corrected in the present edition, and removed in a subsequent one. No possible evil can result to the medical public from fostering and maturing this national work. We grant that it is imperfect, and so are our public standards of almost every description. Our alphabet and our language, our commercial weights and measures, the scale of our thermometers, the artificial divisions of our year, are all, to a certain degree, imperfect and inconvenient; yet, once adopted, the inconvenience and imperfection are no longer felt. If we will wait for a perfect pharmacopœia, which all the world shall applaud; such an one may arrive with the millennium, but cannot be expected sooner. If we will reject a public standard, because it is not what individuals would have made it, we shall for ever remain without one. Necessity will induce separate societies, as they had already begun to do, to form their own standards of nomenclature and preparation; and we may, in a few years,

have our Pennsylvania pharmacopœias, New-York pharmacopœias, and Kentucky pharmacopœias: and it will be difficult to understand the writings of future American medical authors, as it now sometimes is of British, till we have ascertained to what college, school, society, or pharmaceutical sect they may happen to belong.

Your reviewers, gentlemen, if they intended to have influence with the public, should have kept out of sight any local jealousies or private ends which they had to gratify. Notwithstanding the unfortunate lengths to which medical warfare has of late been carried, in some of our central cities, you may rest assured, that the great mass of the medical profession, who stand aloof from interest, or participation in such contests, still retain the power of judging independently and correctly on subjects of general concern. And your reviewers will, I trust, find that the general ends they profess, will not be promoted by the private and local motives they have permitted themselves to disclose. I allude to the expressions of personal hostility to myself, conveyed in various pages of these reviews; partly in remarks on the editorial department of the work; partly through the medium of strictures on different *books*, which neither have had, nor can have, any bearing on the character of the Pharmacopœia; and partly by the dishonourable statement of one of your correspondents, who, in commenting on the "important omissions" of the work, informs the public, that "much of the reprehensible faultiness of that portion of the present work which relates to the American Materia Medica, is to be laid at" my "door." I should deem it beneath me to reply to a charge, the contrary of which is known to all who have been concerned in the formation of the Pharmacopœia. But, as I am desirous to exonerate the Pharmacopœia from any displeasure it may labour under on my account, I beg leave once for all to state, that I was never present at the convention where the Pharmacopœia was formed, and could have no voice whatever in the insertion or omission of any articles of the materia medica; that when, contrary to my expectation or desire, I was placed on the publishing committee, and a subordinate share of the labour allotted to me in preparing the work for the press, I endeavoured

to execute the will of the convention by giving my aid, in conjunction with another gentleman, to put in form those portions, and those only, to which my attention was required. And if it be necessary to make farther explanations, I would, through your journal, inform the public, that the only portions of the American Pharmacopœia which were prepared north of the cities of Philadelphia and New-York, were the prefatory remarks, and the arrangement, illustrations, and references of the materia medica list, including the vegetable nomenclature, which your reviewers condescend to say possesses advantages over those of London and Edinburgh. The chemical nomenclature, or at least that portion of it which relates to placing the base of a compound body at the beginning of the name, was established by express vote of the convention, from which the committee had no right to depart.

As to the province of *editor* to the work, by which title your reviewers think proper to distinguish the individual, on whom devolved the task of correcting the proof-sheets during the printing of the book; the unnecessary rigour of your correspondent's remarks compel him, in justice to himself, to state, that he never saw the final manuscript of the Pharmacopœia until the time agreed on for commencing the printing had arrived and past, and the press, of course, had been waiting for the work; that when this manuscript came at a late period from the south, it was found written in several different hands, some of which were far from being of the most legible kind; that it came at a period when he was particularly occupied by official and professional duties; and that the long previous delay, and the arrangements of the publisher, made it necessary to commit the first part of the manuscript to the press before he could fully compare it with the subsequent parts. Under these circumstances, he might justly be held responsible for the correctness of any part which had been originally written out by himself. In parts written by others, he was furthermore responsible for a general conformity of the printed texts to their written manuscripts. Even farther, he might be held answerable for the orthographical and grammatical correctness, both of the Latin and English; also for the correction of inaccuracies, and the reconciliation of discrepancies, which were "not of a

debatable nature." That these duties were executed by him laboriously, and to an arduous extent, especially on some portions of the work, the printer can bear abundant testimony. That some, I will say many errors, should have escaped his eye, under the difficulties which have been stated, will be admitted as unavoidable by any one who has ever experienced the labour of following the press through a technical manuscript of disconnected parts, a table of numbers, or a catalogue of references. But for an editor to have availed himself of his power with the printer to alter any of the leading or important features of the work, even though at variance with his own conviction and judgment; or to have omitted any portions from its pages, which had been formally adopted by the convention, and written out by members of their publishing committee, appointed to take charge of particular departments; would have been an unwarrantable license, neither to be expected in him, nor justified.

I would now, gentlemen, apologize to your first reviewer for having been obliged thus far to couple him in any way with your second. The spirit of the two productions is in some degree the same; the talent with which they are executed, widely different. To your first reviewer I would readily accord the praise of having studied his subject, and of having pointed out some judicious corrections. But before indulging in the acrimony of remark which occurs towards his close, he should in charity have remembered, that it is less arduous to review a printed book, than to correct a manuscript one; that it is easier to sit in one's closet and copy the objections of Mr. Phillips against the London pharmacopœia, than it is to settle disputed points in chemistry; and that reviewers, as well as authors, are not always exempt from the frailty of error. In illustration of these remarks a few of his own criticisms may be very properly examined.

Pulvis antimonialis.—The reviewer objects to this name, and asserts that the proper name is *phosphas calcis et antimonii*. Now it is by no means a settled point in what state the antimony exists in this preparation, and the task of a reviewer is certainly an easy one, if, in doubtful cases, he is only to assert, without proof, the opposite of what is contained in the book reviewed.

The experiments of Dr. Pearson and of Mr. Chenevix justify a belief that the antimony exists in the state of an oxide, and not of a phosphate, and the Edinburgh college have sanctioned this opinion by the name they have adopted for the preparation. But even if the probabilities on either side were equal, no more convenient and unexceptionable name could be found, than that adopted by the London and American pharmacopœias.

Antimonium tartarizatum.—In this most important preparation, your reviewer again cuts short all doubt, by pronouncing in favour of the name *tartras potassæ et antimonii*, or *tartras antimonii*; and asserting, that as the word *tartar* no longer forms a part of chemical language, consequently nothing can be properly said to be *tartarized*. Your reviewer has probably not attended to the opinion of Gay Lussac, lately confirmed by Dr. Paris, that in the various metalline compounds of which the supertartrate of potass is an ingredient, this latter substance acts the part of a simple acid; an opinion which receives much support from the great solvent property of cream of tartar, and from the striking fact that it is even capable of dissolving various oxides, which are insoluble in tartaric acid, of which the protoxide of antimony is an example. In the present state of our knowledge respecting the composition of tartar emetic, and the uncertainty which exists as to the mode of combination of its ingredients, Dr. Paris observes, “it must be admitted that no name can be more appropriate than *antimonium tartarizatum*.”

Hydrargyri nitrico-oxidum.—“This name for red precipitate,” says your critic, “has been very injudiciously copied from the London pharmacopœia. The prefix *nitrico*,” says he, “appears to be introduced into it under the impression that a small portion of nitric acid is essential to the constitution of the preparation in question.” Now, in truth, the prefix *nitrico* is introduced under no such supposition. It is used by the London college to distinguish this preparation from the other red oxide prepared by heat. Like the adjective terms *precipitatus*, *sublimatus*, &c. often used in pharmacy, it has nothing to do with the composition, but only with the mode of preparation. The name in fact furnishes a very neat mode of conveying what the Edinburgh college have expressed by the cir-

cumlocutory appellation *oxidum hydrargyri rubrum per acidum nitricum*. Yet your reviewer will have it that it is "clumsy," and says "the term *oxidum hydrargyri rubrum*" would have been much more appropriate. How comes he to be ignorant, that there is another medicinal preparation of mercury, bearing exactly the name he proposes, especially, when in another place he has abused the Pharmacopœia for the commission of this very kind of oversight? Really, it does not appear, even if the convention had been enlightened by the presence of your reviewer in person, that they would have been proof against all blunders.

But besides, there is an additional reason why the nitric acid need not be lost sight of in naming this preparation. As it is often prepared, it is actually a *subnitrate*, and if boiled in six times its weight of water, the liquor, when filtered, yields a precipitate with ammonia.

Aqua calcis.—The public would be indebted to your reviewer, if he would let alone this simple preparation, and suffer apothecaries to boil their water to expel any carbonic acid it may happen to contain. It will doubtless afterwards cool to the utmost extent of his wishes.

Hydrargyri oxymurias and *hydrargyri submurias*.—It is not pretended that these are the appropriate chemical names, but they are established pharmaceutical names. The first has the sanction of the London college; the last, of the London, Edinburgh and Dublin colleges. However indifferently a chemist may regard the confounding of these medicines, I believe no practitioner or patient who is concerned in giving or taking them, would willingly see them both called *muriates* of mercury, and trust for their distinction to a subjoined epithet very liable to be abbreviated and mistaken. The terms *chloride* for calomel, and *bichloride* for corrosive sublimate, or the French names *proto-chloruret* and *deuto-chloruret*, might have been adopted; but in the present unsettled state of chemical nomenclature, the convention could not have been justified in departing from the received names of the other pharmacopœias, which are now in use wherever our language is spoken.

Your reviewer deserves well of the public for the industry with

which he has brought to light orthographical errors, superfluous words, and occasional want of correspondence between different parts of the work. As these mistakes are generally not of a questionable nature, it is presumed that, once pointed out, they will be hereafter corrected. Every one will agree with him that the application of the term *aqua potassæ* to two different liquids is an oversight requiring the speediest correction. This may be done by calling the latter *liquor potassæ*. It will also be readily conceded, that a uniform term would have been desirable to express pure aqueous solutions, especially of saline, earthy, and alkaline bodies. I cannot however admit with him that the word "aqua" is the proper name. This term is used by the London, Edinburgh, Dublin and American Pharmacopœias, to express *distilled waters*, a very different kind of preparation. For the solutions of which we have been speaking the London college employs *liquor*, which is an unexceptionable term. The Edinburgh uses *aqua* and *solutio*, which last, as a Latin word, means properly the act of solution, rather than the liquid produced. The Dublin uses *liquor* and *aqua*. The assertion of your critic is wholly unfounded, that "a solution borrowed from the London college is called *liquor*, and one taken from the Edinburgh college termed *aqua*." Witness the solutions of muriate of lime and that of muriate of barytes.

In questions at issue between Mr. Phillips and the authors of the London Pharmacopœia, your reviewer has pursued a very simple course, that of taking the side of Mr. Phillips whenever the convention have not. Perhaps, for the benefit of pharmacy on both sides of the Atlantic, he will hereafter publish his own experiments by which he has been enabled to set at rest these debated topics.

The criticisms of your correspondent on the subject of weights and measures, are particularly entitled to attention. Without feeling specially attached to either mode of admeasurement, I would remark, that something may be said in favour of a system of fluid measures, on the score of its convenience. We may compound and prescribe liquids by weight, but we must administer them by measure; since patients are not often provided with

an apparatus for weighing liquids. And a physician, who directs an extemporaneous compound of different liquids, if accustomed to prescribe by volume, can more readily assign the dose by some convenient measure, than he could otherwise do by calculating the specific gravity of the compound. But, whichever be the standard to be preferred, there can be no doubt that the American Pharmacopœia has been unfortunate in the application of its standard to use. The scale of weights being made to correspond with that of Troy weight, while the liquid measures are divided by the Avoirdupois scale; a change of strength and proportion is caused in many former preparations, the weights of which have been altered to fluid measures. This is particularly the case with those adopted from the Edinburgh college. Here, then, is an important defect in the first edition, and one not immediately to be remedied, since a great part of this edition has gone into circulation. It is suggested, however, as the best mode of obviating this defect, that the president and secretary, with whom the residual power of the convention now rests, if they are satisfied that the convention intended any number of preparations to answer to those of the Edinburgh college, should issue an extra sheet or page containing a list of all such preparations, as well as of any others similarly situated; and requiring of apothecaries to substitute *weights* for *measures* in such preparations; or to correct their books by erasing the word *fluid* in these instances, and putting *pounds* for *pints*. In another edition uniformity might be introduced by approximating computations. The same extra sheet may form a vehicle for any other necessary corrections.

The arrangement of words in the chemical nomenclature has the objection, that it departs from the order most common in modern languages. It has the advantage, however, that in the *Materia Medica* and Latin index, it brings together the preparations of each metal, alkali, earth, &c. which would otherwise be separated under the names of various oxides, salts, the prefixes *sub*, *super*, &c. To persons conversant with Latin, either arrangement of the name is equally convenient.

As to the second review, gentlemen, which your journal contains, both the ability and the regard to truth with which it is written, are of too humble a nature to merit long attention. It has been facetiously laid down as an accomplishment in a reviewer, that he should be able to review a book without reading it; but your correspondent has certainly disclosed a more novel accomplishment; that of reviewing a book without understanding the subject or science, about which it treats.

This reviewer, in his simplicity, informs us, that he thought the convention were going to form, not a pharmacopœia, but a *national dispensatory*; and thinks the public must be greatly disappointed that such a work has not been produced. For aught we know, others, equally wise, may have expected a system of anatomy, or one of therapeutics. But it seems the convention, not being seasonably enlightened on this subject by the reviewer, construed their duty differently.

After a long, and not remarkably instructive, disquisition on American literature, and "British intolerance" of it; diversified with regrets that the Latin language should have been admitted into the *Pharmacopœia*, and lamentations over the "contempt" into which certain collateral sciences, from some unaccountable cause, have fallen in certain universities; the reviewer proceeds to point out various errors and their corrections, truly worthy of his talents to discover and amend.

The accidental repetition of the important article *marjoram* affords him materials for a paragraph; and in the omission of *pennyroyal*, which happens to be inserted in one place, and left out in another, he discovers a deep laid conspiracy to put down a certain Philadelphia book which is said to contain it.

He tells us, "the *extractum cicutæ*, extract of cicuta, has been uselessly changed into *extractum conii*," and in English, "extract of hemlock." From what, pray, has this useless change been made? Not from the London or Edinburgh pharmacopœias, for both these call the plant in question *conium*. Not from their translators Powell, Duncan and Thompson, for all these use the English word *hemlock*. Should your reviewer hereafter be led to the study of botany, he will find that *conium* and *cicuta* are

names of different genera of plants, and have been so since the days of Linnæus, and that to designate an article drawn from one, by a name drawn from the other, would be indeed a "useless change."

In like manner he stigmatizes as "innovation" the translation of *stramonium*, which has been in use from the days of Gerard's Herbal, down to the Edinburgh Dispensatory. And with an intuitive talent for deciding all questions of vulgar nomenclature, he condemns *thoroughwort*, and declares in favour of *boneset*. Had the convention adopted *boneset*, he would probably have declared for *thoroughwort*.

In regard to *tincture of peppermint*, this critic assures us, that the convention "will not find it easy to convince any one who reflects on the subject narrowly, of any use in calling that a *tincture* which has heretofore been invariably denominated a *spirit*, or commonly *essence* of peppermint." Now, if he will reflect narrowly on the subject himself, or rather if he will peruse the common books on pharmacy, he will find, that the spirit of peppermint of former pharmacopœias is made by distilling spirit or diluted alcohol from the plant itself, as is done in other aromatic spirits; whereas the present preparation, or *tincture*, is made without distillation, by digesting the oil of peppermint in alcohol till it is dissolved. The *tincture* thus prepared is a much more certain preparation than the *spirit*.

Under the article *horse-radish*, he complains, that "the plant, meaning the herb of course," is retained to the exclusion, he wisely thinks, of the root. As it appears that this reviewer is not a botanist,* it will be useful to him to know, that a *plant*, according to Linnæus, is divided into the *root*, *herb*, and *fructification*; and that the word *plant* does not mean *herb*, any more than the word *animal* means *head* or *trunk*. He will also notice, that in a pharmacopœia, when the word *plant* is used on the materia medica list, the whole plant is thought worthy of being preserved. Thus,

* This is incorrect. The review bears evidence, not to be mistaken, to the contrary. For botanical knowledge and professional erudition, the reviewer holds, indeed, a very distinguished rank. The instruction offered is, therefore, entirely misapplied. EDITORS.

in cassia marilandica, it being the opinion of members of the convention that the root is medicinal in common with other parts of the vegetable, the *plant* is directed to be kept.

So well pleased is your critic with the exercise of correcting errors, that where he cannot find points really objectionable, he proceeds to manufacture imaginary faults, that he may enjoy the pleasure of setting them to rights. His labours would probably have been more useful, had he taken care to distinguish between his corrections in theory, and corrections in fact. Thus, when he tells us (p. 516,) that "*aralia spinosa* should have been named *angelica tree*," he should have added, *provided, nevertheless, it be not so named already*. And (p. 514) where he quotes "*veratrim viride*" as the spelling of *veratrum viride*, he should have subjoined, *provided, always, that it should ever have happened to be so spelt*. Thus would the American public feel the satisfaction of possessing a reviewer, whose talents extend to the cognizance and correction, not only of errors which have been, but likewise of those which might have been, or from henceforth may be, committed!

The palpable deficiencies of the Pharmacopœia are illustrated by a long list of articles, which would probably have been introduced, had your reviewer had a hand in the formation of that book. The list begins with "*currant jelly, dewberry jam, blackberry jam, raspberry jam, American raspberry jam*," &c. &c. But your readers will begin to exclaim, *Jam satis est*.

Such, gentlemen, is a specimen of the grade of understanding and talent, which is likely to be most clamorous against the introduction of any standard of public utility in pharmacy, which may be proposed. "*A pharmacopœia*," says Dr. Paris, "*is always an object of abuse, because it is a national work of authority, which is quite a sufficient reason why the ignorant and conceited should question its title to respect, and its claims to utility*. Plures andivi, says Huxham, *totas blaterantes pharmacopœias, qui tamen ne intellexerint quidem, quid vel ipse pulsus significabat*."

Apart from the consideration of the National Pharmacopœia, your reviewer has given himself particular anxiety in regard to

another work, not yet published, but announced under the name of "A Sequel to the Pharmacopœia." This work he considers to be "a money-making scheme in the bookseller," and trusts the public will not receive it as a second volume of the Pharmacopœia. Doubtless all books are money-making, or money-losing concerns to their publishers; and generally so, in proportion to their degree of interest and usefulness. In this respect, the projected book must take its chance with other productions of the day. And whether the public, or any of its members, shall choose to look upon it as a second volume to the Pharmacopœia, or as an explanatory "Sequel to the Pharmacopœia," which is not a second volume thereto; is probably a matter of indifference to the publisher. The expediency of such a work from some individual, it is believed, will be admitted, by those who consider the nature of its subjects. It was first thought of during the printing of the Pharmacopœia; and your reviewer undoubtedly knows, or ought to know, that no portion whatever of the proposed *Sequel* was ever contemplated as part of the Pharmacopœia itself, either by the convention, or by any well informed individual among their constituents. The field is now as open for such a work, as it ever was for notes to a classic, or commentaries on a law book. And, although your critic has troubled himself to do it the good office beforehand, of informing the public that he shall not approve of any such work; the public, perhaps, from the specimen they have had of his powers and acquisitions, will not be particularly anxious that he should.

In regard to the cause of American literature and science, to which this reviewer is so anxious a well wisher, and so particularly useful an auxiliary; it may be remembered, that as a pharmacopœia is a work rather of medical police, than of literary novelty and display, it is one of the last kind of books to affect national literary character. We do not suppose the French nation to be a remarkably illiterate people, because Mr. Phillips has thought proper to call their new pharmaceutical code "a libel upon the age and country which has produced it;" nor is it generally believed that the city of London is relapsing into barbarism, because the same author has seen fit to bestow on its phar-

macopœia epithets equally decorous and flattering. But our reputation will indeed suffer, and justly suffer, if, after an effort of public unanimity and sacrifice, unprecedented, perhaps, in the medical history of Europe, we shall voluntarily deprive ourselves of a public good, that we may promote the ends of the selfish, the unreasonable, and the anonymous.

Finally, gentlemen, permit me to call your attention to a subject in which you, in common with the rest of the medical profession, are interested. The present volume, with its future revised editions, is the only National Pharmacopœia which the medical profession in America can ever expect to see. No future convention will encounter the expense and sacrifice of a meeting, if the failure of a previous attempt is before their eyes. Nor will individuals leave their own concerns, to labour for those of the public, if they are to be singled out, in consequence, as marks for personal calumny. The first edition of the Pharmacopœia has been examined, its defects admitted, and the modes of remedying them pointed out. This edition cannot be suppressed, even if this were desirable, since two thousand copies have already gone into circulation. Many, and it is hoped all, of the societies which have co-operated in its formation, will give their consistent support to its adoption and gradual improvement. I may cite the example of the Massachusetts Medical Society, a body composed of all the regular physicians in that state, who by express vote have given up their own excellent pharmacopœia, founded on that of Edinburgh, and adopted the national standard; not from any conviction of its superiority, but from a desire to redeem the pledge they had given to the rest of the medical public, to co-operate with them in adopting and maturing a national standard of pharmacy. It cannot be doubted, that all our medical societies, which have a true understanding of their own and the public good, have pursued, or will pursue, a corresponding course. This will lead to the eventual correction of all the important errors and defects of the work. Those parts which depend on opinion merely, or relate to unsettled questions in science, cannot be, or at least ought not to be, altered, while there is no certainty of changing them for the better, or to more general

acceptance. In the mean time, whatever sacrifice of individual habits and preferences may be called for, in the introduction of this new standard, it will soon be forgotten in the security and convenience of a universal and intelligible language of pharmacy.

POSTSCRIPT.

SINCE the above was written, I have met with another review of the American Pharmacopœia, in the *Philadelphia Journal of Medical and Physical Sciences*. If proof was wanting before, these criticisms sufficiently demonstrate, that no pharmacopœia, of whatever character, and under whatever circumstances, could possibly have given satisfaction, or appeased the predetermined hostility of individuals resolved not to be satisfied. I pray you, if you have any patience, to collate a few parallel passages from the three several reviews which your city has produced; and imagine, for a moment, what kind of a book the reviewers would themselves have made, had they met in conclave for the purpose. One condemns the Pharmacopœia for its useless redundancy; another, for its unpardonable deficiency. One complains, that it is not written wholly in Latin; another, that it is not written wholly in English:—it is written in both Latin and English, and yet neither is satisfied. One inveighs against the attention which has been shown to indigenous medicines; another is indignant that so little attention has been paid them. One is angry that the convention have adopted Mr. Phillips' mode of making tartar emetic, and considers, that as none of the British colleges have attended to him, he is no authority; another considers him as quite oracular, and blames the convention for not following him in every instance. One pities the bookseller for having bought the copy-right of so worthless a publication; another threatens to contest with him in a court of justice the validity of the title, by which he presumes to monopolize the profits of the work he has bought!!—Surely, never was any production of the press so unmercifully set upon by hungry pursuers, and with so hopeless a chance of obtaining quarter.

How is it, gentlemen, that when the rest of the medical profession, throughout the union, seem disposed quietly to receive and mature the standard of pharmacy which their delegates had prepared for them, the writers of your city are thrown into so unnecessary an agitation on the subject? It is not because the *Pharmacopœia* is a different work from what it should have been; for your critics are not agreed in any distinct conception of what such a book ought to be. Nor would I for a moment suppose, that it can arise from so unworthy a motive as the consideration, that neither "the University of Pennsylvania, nor any medical body of (your) city, except the College of Physicians, was represented on this occasion; for it is well known, that the most public and earnest invitations were made to all medical bodies, to co-operate" in the design. Least of all, would I be willing to admit, that any private pecuniary interests, incompatible with the sale of a national pharmacopœia, can have produced opposition to so great a national benefit. It is indeed impossible to admit, that the voice of these reviewers is any index to that of the more enlightened physicians and apothecaries of your city.

The invectives which have been so bountifully and precipitately bestowed on this production, will no doubt prove to it of ultimate advantage. Whatever temporary depression a work may experience from unfair and illiberal aspersions; when the grounds of such aspersions have been examined, and duly appreciated, a reaction is apt to take place in the public mind, more permanently useful to the object of persecution, than any premature excitement in its favour could have been.

There seems to be in all the reviewers a misconception of the real nature and intention of an American pharmacopœia. It is not to produce, or invent, a set of preparations of the most perfect possible kind; for such an end is altogether unattainable. But it is to designate a competent number of useful medicines by certain specific and determinate names; so that, throughout the United States, these names shall produce these medicines, and nothing else. One or two examples will illustrate these positions. Take any common preparation in the book; for instance, the old Dover's powder, composed of a grain of ipecac., a grain of opium,

and eight grains of sulphate of potass. Who will undertake to say, that eight grains of sulphate of potass are better than seven grains, or than a dozen grains? Or who knows that sulphate of potass is any better than sulphate of soda? Small minds might write volumes to prove either side of the question, without bringing it any nearer to a conclusion. But framers of pharmacopœias must adopt something; and by fixing on a determinate formula, they furnish the profession a convenient and useful medicine, the strength of which is not liable to deceive them. Again, tincture of opium, in the American Pharmacopœia, is made by a certain combination of certain ingredients. When the Pharmacopœia has become generally adopted, every practitioner knows that every honest apothecary will furnish him laudanum of a determinate strength, so that he can estimate its dose and calculate its effect. Now, of what consequence is it, whether this is the best possible mode of making the preparation; or whether laudanum would not, on the whole, be better, if made a little weaker, or a little stronger? Of what consequence is it, that reviewers, either at home or abroad, are in a state of commotion on the subject? It will neither deprive the patient of his nap, nor the physician of his dinner.

If the American Pharmacopœia establishes several hundred medicines by specific and unchangeable names, it confers a lasting benefit on the public, which every physician will feel in every day's practice. Admit, that among these there should be forty or fifty preparations too faulty to be used. They will doubtless be corrected when such faults have been pointed out. Admit, that forty or fifty more do not suit the taste of capricious individuals. Such individuals are not obliged to employ them. The right of extemporaneous prescription is as open as it ever was. But let them not attempt to disorganize and undermine, what they themselves could not have improved. Let them not gnaw the roots of a tree, which the community would wish to see flourish. Let them not set fire to a national edifice, because the style of its architecture does not agree with their own undefined and fastidious notions.

The reviewer, in the Philadelphia Journal of Sciences, it is
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true, professes to admit that "it is this uniformity of preparations which alone renders a pharmacopœia desirable." Yet, the whole spirit and tenor of his review shows, that to him at least, a national work establishing such a uniformity, is not desirable. Without condescending to admit that the present work, composed, as it is in a great measure, of formulæ selected from other pharmacopœias of respectability and long standing, must possess some merit, and deserve some respect; he proceeds to assail it with unsparing hostility, and to heap upon it, without discrimination, objections new and old, correct and unfounded, decent and ridiculous. He attacks, with implacable severity, the general features of the work, the positions of the preface, the application of the names, and the detail of the preparations; and finally declares, that unless the convention will do an impossibility, he will cheerfully unite in any proper measure by which the work may be set aside.

There is no end to the objections which a dogmatic and unreasonable criticism may heap upon any work, the subjects of which are of an unsettled and disputable nature. The Philadelphia reviewer, at the expense of contradicting himself, in one place presumes that the convention repeated many times every process they have recommended, with the same care which Dr. Hope of Edinburgh professes to have been used in that city; in another, he incredulously asks, if any of them ever made tartar emetic by the process they have recommended, and which of the formulæ has been submitted to the experimental researches of the convention or its committee. Now, either reason or inquiry might have convinced him that both his assumptions are unfounded. The convention could not repeat the processes of the Pharmacopœia; yet in so emphatically important an article as tartar emetic, a new formula would not have been adopted without both a chemical and medical investigation of its character. He cavils with the direction given that certain compounds are to be made by the apothecary alone, and tauntingly asks, what apothecary in the United States, who is not a manufacturing chemist, ever pretends to make them? and thinks that a majority of them must acknowledge their total incompetency to the task.

I would ask, which is most conducive to the public safety, that the apothecaries, as a profession, should be expected to make these compounds, or that they should not? In the former case, the apothecaries would at least *know how* to make them; and though, from considerations of convenience and profit, the making of certain articles may be confined to a few, yet the rest will be better judges of the articles, than if they had never troubled themselves about the ingredients and processes. However contemptuously the reviewer may view the profession and duty of an apothecary, there are few classes of men whom it is more for the interest of the public to have generally and thoroughly educated, than these. Suppose he had seen fit to ask, what physician in the United States "ever pretends" to prescribe for the plague? Would it follow that a knowledge of the plague should form no part of our medical education? There are as many apothecaries in the United States, in the course of a year, who make ammoniated alcohol, magnesia, caustic potass, &c. as there are physicians who prescribe for small pox or hydrophobia. Yet the reviewer would not have apothecaries liable to be called on to prepare such articles.

There are various parts of this review which might be answered at length, were they of sufficient importance to deserve the time. When the reviewer denies, that the *usefulness* of a pharmacopœia is proportionate to the extent of its circulation; I would ask if, *cæteris paribus*, a national pharmacopœia is not likely to be more useful than a state pharmacopœia. Books of this sort, like money, or like iron, may have an intrinsic value, dependant on their capability of doing good; but they are not *useful*, except in proportion as they circulate. When he asserts, for the mere pleasure of contradiction, that the London pharmacopœia is confined to England, the Edinburgh to Scotland, &c. does he believe that these works are restricted to certain book-stores; or that a London physician wishing for an Edinburgh preparation could not obtain it in his own city? Apothecaries in London may be required to keep the articles of the London college; but they may also keep what else they please, for the accommodation of whom they please. Will the reviewer inform us

which pharmacopœia is the one used in Calcutta, Jamaica, or Canada?

In his criticisms on nomenclature, he condemns the names of certain preparations of mercury, and thinks the terms *calomel* and *corrosive sublimate* should have been preserved. He will, perhaps, admit, that as these appellations, together with many others of their kind, are retained in the Pharmacopœia as synonyms, they may be used by any who prefer them, without danger of mistake. His critique on the term *acacia*, shows that he is ignorant of the ground on which the London and Edinburgh colleges have adopted it. But there is no end to idle criticisms about names, especially by a reviewer who does not know his own mind on the subject, and remarks on one page, that "There is no excuse for retaining old names, if improper, when so good an opportunity offered of removing the trammels of other countries;" and on the opposite page—"yet if names are perpetually changing, either from fancy or some presumed superiority, how is the learner of our science to ascertain precisely the substance intended?"

There is little hope for a book of science in the hands of a critic, who, with a dignity becoming the nature of the subject, sets in array against it such respectable assailants as Homer Travestie and Monsieur Tonson; and who counts over the number of preparations, that he may instruct the profession by his geographical and chronological coincidences. Thus he has discovered, that there being fifty-two *tinctures* in the Pharmacopœia, it makes "one for each week in the year;" and, that "if the convention had given us only three more *ointments*, we might have had the pleasure of denominating one after each of the United States." How different, he might have added, is the American Pharmacopœia from that of Edinburgh, which, having only seventeen ointments, would require five more to bring it up to the same ominous number! Objections of this sort, it must be confessed, are wholly unanswerable; and it would be unfair to attempt saying any thing which might tend to lessen in the author's mind the triumph he undoubtedly feels, at having conceived so profound and brilliant a thought.

The uneasiness of the reviewer in regard to the copy-right of the book, may be allayed, if he will recollect that from the first

moment that the plan of a pharmacopœia was formed, it was announced that the proceeds of the work were to go towards defraying the expenses of the convention. The proceeds have already been applied to that object, and found somewhat less than sufficient; so that it is trusted the convention will be exonerated from blame on this head. A public advertisement was made before the manuscript was sold, and it was particularly offered to the proprietors of the different dispensaries. And finally, the purchaser having taken out the copy-right in his own name, he will undoubtedly hold it for the period of his contract, against all persons who cannot show that he came by it fraudulently.

It is hardly worth while to return to the rusty topic which this reviewer has called up again, of foreign critics and American literary reputation. There is little doubt that Mr. Phillips, the self-constituted judge and condemner of all pharmacopœias; who has successively written down those of England, Scotland and France; will accommodate us with a *phillipic* adapted to American latitudes. But if we shall stand the shock, as well as our brethren in Europe have done, no great national calamity need be anticipated in consequence.

I have only time to answer one or two inquiries made by this reviewer.

Question by the reviewer.—"What shall we say of an ointment of rose water, in which the water must necessarily escape, leaving only a simple cerate slightly impregnated with the odour of the rose?" *Answer.*—The water does *not* escape, but remains incorporated with the other ingredients, forming an elegant and useful preparation, extensively employed.

Question by the reviewer.—Quere, If *singulis octantibus* be correct Latin? *Answer.*—Very correct indeed, as is also *unciis quatuor*, and some other phrases, marked down as errors by the reviewer. Had he taken the trouble to consult his Latin grammar, before he consulted the public, he might, at least, have been spared the trouble of adding his own mite to the mass of American literary ignorance so much complained of.

ART. XI. *A Reply to the Remarks of J. on the Review of the Papers relating to the Fever in New-York, in 1820.**

THE review of the documents relating to the fever which prevailed in Bancker-street, (New-York) during the summer and autumn of 1820, exhibited so complete a refutation of the report in which this disease was declared to be yellow-fever, and gained so signal a triumph over the committee who had thus attempted to destroy public confidence in the Board of Health of that city; that it was confidently expected, that no other advocates would be found for the mischievous doctrines which they had advanced. Dr. Jackson of Philadelphia has, however, presented himself as the champion of this humiliated cause, which even its original advocates have not ventured to defend; and in the last number of the Recorder, has laboured to subvert one of the capital positions of the reviewer, and to convict him of ignorance, immodesty, and unfairness. I should not have taken upon myself to expose the emptiness and disingenuousness of his reasonings, were it not that he urges his arguments with a dogmatism well calculated to command the belief of the unwary; and with a pomp of language, and a parade of erudition, which may even captivate readers of some intelligence and shrewdness. The only apology I have for again obtruding upon the reader a subject which has already excited so much turbulent disputation, is my conscientious conviction, that the committee of the Medical Society of New-York, have published to the world a most unwarrantable attack upon the public authorities of that city, and promulgated doctrines as mischievous as they are delusive; and that it is the solemn duty of every one who knows and values the truth, to exert himself in every possible manner to destroy the influence of a faction so dangerous, and who, for the gratification of private malevolence, and the support of a tottering theory, have not hesitated to assail a body, whom it is their duty to assist, and whom every virtuous citizen cannot but respect.

* See page 419 of this volume.

Without further preface I proceed to examine Dr. Jackson's "remarks" upon the review ; confident that I shall be able to show, that he has entirely failed in his attempt to refute the position of the reviewer, and to justify his own "swaggering" assertion that "*all* experience, and *all* authority, establish the reverse position as true."

It may not be amiss to make a passing remark upon the high sounding prolegomenon, "*multa et præclara minantis*," which serves as introduction to the paper. I confess I cannot see its relevancy, or discover its connexion with the subject of the piece. It looks as if it had been a choice scrap written for some other occasion in the author's common-place, and which he thought too good to be any longer withheld from public admiration. He has in it, however, one excellent remark, which perhaps may explain all the controversy that the subject has excited. After telling the reader that one party, of whom the committee of the Medical Society were the ring-leaders, had pronounced the Bancker-street disease to be yellow-fever, and that others contended it was typhus ; he very truly says, that these diseases "cannot be mistaken or confounded, when *soberly* examined." Had this rule been strictly observed by the "*veterans*" of the committee, their decision would, I am sure, have been of a very different nature.

Dr. Jackson begins his remarks by accusing the reviewer of being ignorant of one of the commonest and best established facts in medicine, and of writing with a fluency proportioned to his deficiency of information. It is impossible that he can believe this. If he has any judgment, or any candour, he must acknowledge that the review is written with no mean ability, and exhibits no common share of medical learning. But this is only another example of the "swaggering" dogmatism which so often appears in the writings of Dr. J. Can he seriously think that petulant declamation will be received for argument ?

The committee of the Medical Society, in their report, laid it down as one of the characteristics of typhus, that it does not prevail in warm weather ; and even pronounce it "repugnant to reason and common sense" to call the Bancker-street disease ty-

phus, because typhus is "a fever generated in a cold, and extinguished by a hot temperature." Dr. Jackson too, in "the Philadelphia Journal of the Medical and Physical Sciences,"* says, that "*all* experience, and *all* authority, establish the reverse position as true, and typhus is admitted by the most eminent and skilful of the profession, to be abated and destroyed by the heats of summer, and to flourish and be rife in the colds and damps of winter." The reviewer asserted, and endeavoured to prove, that cold weather was by no means *essential* to typhus, but that it might, and frequently does prevail, in the heats of summer. I conceive, then, that Dr. Jackson is bound to show that typhus *never* prevails in warm weather. This is fairly the point at issue between him and the reviewer, and not whether typhus disappears in warm weather, or is most common in winter.—But let us see whether the authorities he has adduced, go to establish this position; and whether he has justified his assertion, that "*all* experience, and *all* authority," prove that "typhus fever *never* prevails in hot weather."

He first adduces the authority of Sir Gilbert Blane. But from the very quotation which he gives from this author, it is evident that Sir Gilbert does not think it *impossible* for typhus to prevail in warm climates, since he acknowledges that although tropical heat is unfavourable to its extension, it *may be continued or revived* even there by neglect. That "uncommonly long and severe cold weather" is favourable to the generation and propagation of typhus, no one denies. This was never made a question by the reviewer. He contended that the committee were wrong in restricting typhus to the winter season; and that Dr. Jackson was equally wrong in saying, that "*all* experience, and *all* authority" justified their position. But let us examine the opinions of Sir Gilbert Blane, somewhat more particularly; and we shall find, that, so far from countenancing Dr. Jackson in his curious notion about typhus, he contradicts him in the most pointed and unequivocal manner. In his "Observations on the Diseases of Seamen," he tells us of the existence of a low ship fever on board of two vessels on the West India station, in the June of 1780.† He notices facts of the same

* No. 2 p. 338.

† London edit. of 1789, p. 32.

kind, as occurring in the May of 1782 ;* in the June† and July‡ of the same year ; and in the July of 1783.§ In fact, almost every chapter contains some account of jail or ship fever prevailing in tropical climates, and in hot weather. Dr. Jackson certainly can never have read this book. If he has, (and he would make us believe that he is familiar with all the standard authors,) I do not hesitate to charge him with “handling it *deceitfully*.” If he has not, he is scarcely less culpable for the bold and positive manner in which he speaks of it, and for the false representation he gives of the sentiments of its author.

Dr. John Hunter is next adduced. But the first quotation from him, only tells us that he has *seen* typhus in the winter ; and the second goes to show what was never questioned, that it is most likely to prevail at that season. Lest the reader should think I am unfair, I will give the very words as quoted by Dr. Jackson. “In the *winter* of 1779–80, I had an opportunity of enlarging my experience on this subject, (typhus,) and again in the *winters* of 1783–4, and of 1784–5.” Again, “I have never seen the fever earlier than the month of November, and I believe it seldom appears so soon. It becomes frequent about Christmas, and increases during the months of January and February. If March and April are warm, it grows less frequent ; but if they are cold, it continues nearly as common as in the preceding months—when the weather begins to grow warm, it gradually disappears.” Can any candid person see any thing in all this which disproves the possibility of typhus fever’s prevailing in warm weather ? The whole amount of Dr. Hunter’s words, is, that he has *seen* typhus in the winter, and believes that it *seldom* appears earlier than November. Nay, by his very expression, he would seem to admit that it sometimes does. I contend, then, that nothing can be extorted from Dr. Hunter, even by the most subtle powers of perversion, to justify Dr. Jackson’s assertion that “*all* experience, and *all* authority,” prove that “typhus *never* prevails in hot weather.”

Dr. Trotter furnishes the next argument. But what does he

* London edition of 1789, p. 114. † p. 126. ‡ p. 136. § p. 383.
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say? He doubts if typhus is ever seen within the tropics; he says that it *generally* appears in a cold climate or season; that it is *most apt* to prevail in cold weather; and that *if it should appear in summer*, it is more easily subdued. I refer the reader to the quotations from Trotter, as given by Dr. Jackson, that he may convince himself that I am not the least unfair in this representation. And now I would ask Dr. J. himself, how Dr. Trotter's *doubting* on the subject can furnish him an argument to prove that his position "is one of the commonest and best established facts in medicine?" But Dr. Trotter clearly acknowledges the fact of typhus fever's occasionally prevailing in warm climates and seasons, by the very expression of his belief that it is *generally* otherwise, and that it is *most apt* to prevail in cold weather. But he does not leave us to a doubtful inference; he positively declares, that it may appear in summer, but that it is then more easily subdued. Again, p. 196, Vol. I. he tells us of the existence of typhus at Spithead, and immediately adds: "It was now summer, and the weather uncommonly warm in England." Again, p. 197, in immediate continuance with the last quotation given by Dr. Jackson: "The fever which was spread from the French prisoners to our seamen, after the battle on the 1st of *June*, appeared in the first attacks about the 8th and 10th of the same month. There were, perhaps, never an instance of contagion becoming so general in so short a time." He says immediately after, "the heat was greater at this time than I ever remember to have known it." Speaking of the disease in another place, p. 258, the same author remarks, "in some bad cases of the typhus which occurred among our people, after communication with the French prisoners, there seemed to be a greater quantity of the biliary secretion, than what was usually met with in fevers which appeared in the winter season. The weather was *extremely hot* at that time." And speaking of the treatment of typhus, p. 271, he says: "In *warm weather* the patient may amuse himself out of doors." And finally, as if to make his testimony as complete as possible, he gives us in p. 300, a diary of a fever that appeared on board of a man of war in the July of 1795.

After this view of Dr. Trotter's sentiments, I cannot recognise

the logic which would deduce from his words any inference in vindication of Dr. Jackson's assertion, that "*all* experience and *all* authority" prove that "typhus *never* prevails in hot weather."

The next authority is Mr. William Lempriere. And here it must be acknowledged that Dr. Jackson has been rather more fortunate in his quotations. But notwithstanding Dr. Jackson's opinion of his work, and Dr. Hosack's quotations from him, I can never allow him to be ranked with Blane, and Hunter, and Bancroft, and Armstrong.

The extract from Dr. Bancroft is certainly apposite, and strongly corroborative of Dr. Jackson's opinion. Yet it will hardly be contended that his authority is to outweigh the testimony of so great a host of writers as may be adduced against him.

We come now to see how far Dr. Armstrong agrees with Dr. Jackson. And here I have to reprobate in the most decided manner the unfairness and disingenuousness of Dr. Jackson in quoting Dr. Armstrong just so far as it *suit*ed his purpose, and in suppressing the last clause of a sentence because it directly condemns his theory. It is but just to give the whole quotation, that the reader may see how far the spirit of controversy will influence an infuriate partisan to do what every man of high honour and delicate sensibility must condemn; and how inevitably it is accompanied by the spirit of perversion. I will take the quotation from Dr. Jackson's paper, supplying from the original what he has had the unfairness to omit. "Typhus is unquestionably most prevalent in cold or temperate climates. It would indeed appear, from an author of great research, that this disease had probably not occurred on either side of the Indian peninsula; though it is universally known, that the warmth of tropical regions, is most congenial to the generation of those effluvia, which produce the remittent yellow-fever." So far Dr. Jackson—but Dr. Armstrong immediately continues: * "*I have known, however, practical men who were confident that the contagion of typhus occasionally exists in some hot countries; but they seemed to con-*

* Armstrong's Practical Illustrations of Typhus Fever, &c. American edit. p. 18.

sider, as Sir Gilbert Blane long since asserted, that there is something in those countries unfavourable to its production and continuance." The other quotation as given by Dr. Jackson, is as follows: "In England typhus is evidently favoured by a low temperature, being most prevalent in the cold seasons of winter and spring, generally abating or disappearing, as the heat of summer advances, and often prevailing to a considerable degree in cold wet autumns." But by a most unwarrantable *syncope*, he omits the remainder of the sentence:—"but nevertheless it occasionally prevails at all times of the year, and is even undiminished by the hottest weather in this country, as I once witnessed when the thermometer was unusually high during the greater part of the summer."* After this exposure of Dr. Jackson's expertness at quoting, the reader is, no doubt, prepared to side with me in the opinion that Dr. J. is positively contradicted by Dr. Armstrong; and is by no means supported by him in the sweeping declaration, that "*all experience and all authority*" prove that "*typhus never does appear in hot weather.*"

The last authority advanced by Dr. Jackson, is that of Dr. Veitch. The whole amount of the quotation from his book, is an expression of his concurrence with Sir Gilbert Blane, in the opinion that "the atmosphere of the West Indies is unfriendly to the generation and propagation of *infectious* diseases." The least that can be expected from Dr. Jackson is to make it appear, that Dr. Veitch identifies typhus with infectious fevers. If this is Dr. V.'s intention, the quotation is apposite enough; but until this is established, it certainly cannot fairly be admitted in proof of Dr. Jackson's opinion. But even allowing to Dr. J. the full benefit of the extract from Dr. Veitch, it by no means disproves that typhus may and does prevail in summer in moderate climates; nor does it give the shadow of a sanction to his "*swagging*" asseveration, that "*all experience and all authority*" prove that "*typhus never appears in hot weather.*"

After showing, as I feel confident I have, that Dr. Jackson has adduced authorities to prove what the reviewer never denied, i. e.

* Armstrong's Practical Illustrations of Typhus, p. 18.

that typhus fever prevails, and is most apt to prevail, in cold climates and seasons; I cannot by any means indulge him in the imagined triumph he seems so heartily to enjoy. He has altogether mistaken the point at issue; or, at any rate, has diverted the reader's attention from the question in controversy. I must again insist upon it that it clearly behove him to show, not that typhus prevails in winter and is most common at that season, but that "*typhus never appears in hot weather.*" It is only by disproving the reviewer's assertion that it *does* prevail in summer, that he can entitle himself to the glory of a triumph, and justify his own "swaggering" declaration that "*all experience and all authority*" prove that "*it never does*."

It would be improper not to expose the artifice which Dr. Jackson uses to enlist the party feelings of his readers against the reviewer. After boasting in swelling terms of the demonstration he has given that the Bancker's street disease could not have been, and was not typhus fever; he sneeringly concludes, "that *Dr. Hosack and his coadjutors* have yet to learn some of the simplest diagnostics of our most familiar diseases." Now where was the necessity or propriety of introducing Dr. Hosack in this investigation? Was it not evidently designed to identify this dispute with the controversy in which Dr. Hosack has distinguished himself relative to the nature and origin of yellow-fever, and thus to bring odium upon the reviewer, by representing him as the coadjutor of that gentleman in the defence of an unpopular theory? But the fact is, the reviewer himself is directly *at variance* with Dr. Hosack, and is by no means a believer in the doctrine of the contagion of yellow-fever. The questions are perfectly distinct. And I am persuaded that no sensible and candid abettor of the domestic origin of yellow-fever, will ever refer to the Bancker's street disease as an instance of its local generation. The intelligent men of the party will ever consider the transactions connected with this business as a disgrace to their cause.

But I think I can discern another reason for the introduction of Dr. Hosack's name into this controversy. There is no surer way to raise one's self from obscurity and to secure the gaze of the public, than to set up as the antagonist of a distinguished man. I

shall not here engage in the defence of Dr. Hosack against the pitiful sneers with which his professional sagacity is assailed—"The blood of Douglas can defend itself."—But I cannot but believe that Dr. Jackson, in assuming the part which he has acted in this business, was not altogether a stranger to the feelings of the poet when he gave vent to the ambitious aspirations of his soul in the following couplet:

———"Tentanda via est, quâ me quoque possim
"Tollere humo, victorque virûm volitare per ora."

Virgil. Georg. III. l. 8.

Or, as it is beautifully and pointedly translated by Dryden:

"New ways I must attempt, my grov'ling name
"To raise aloft, and wing my flight to fame."

If this was the motive which dictated his unprovoked attack upon Dr. Hosack, let me remind him, that Zoïlus is known only as the carper at Homer's genius; and that the wretch who fired the temple to perpetuate his name, is remembered only to be execrated.

Having shown that the authorities adduced by Dr. Jackson are for the most part inapposite, and do by no means contravene the position assumed by the reviewer; I now solicit the reader's attention to a few authorities which completely establish the fact that typhus may and does prevail in summer, and directly contradict the flippant assertion of Dr. Jackson, that "*all* experience and *all* authority" prove "that typhus *never* appears in hot weather."

It is necessary, before proceeding to the triumphant testimony which shall be adduced on this point, to remove the objections which Dr. J. has made to the authority of some writers quoted by the reviewer. He evades the strong language of Sir John Pringle by accusing him of being deceived by an erroneous notion as to the cause of typhus, and of being ignorant of the distinctions between genuine typhus and bilious and remittent fevers of a low type. Now I would appeal to the common sense of the reader, and to the candour of Dr. J., to say whether it is not more probable that so accurate an observer as Sir John Pringle confessedly was, would have arrived at the conviction that typhus fevers

prevailed in summer from having actually seen them at that season, than that he should have been deceived into this belief by any erroneous notions about their cause. On the subject of his ignorance, I am sure that every medical reader has anticipated me in reprobating Dr. Jackson's arrogant denunciation of an authority so high. He can never be permitted to anathematize and charge with prejudice and ignorance an author whose writings are to live till the last fire shall consume all learning. A man of real talents may, in the ardour of controversy, be unfair; but he will always be ingenious. He will not deny an established fact, or denounce a standard authority, because they contradict a favourite opinion. He will rather invent a way to reconcile them with his hypothesis.

Dr. Jackson evinces the same unfairness in his objections to the testimony of Willan and Bateman. *They are "English authors,"* and "*treat of typhus as it appears in Great Britain.*" Away with this puerility! Are not *all* the authors quoted by himself, either *British* or *West-India* writers? They are so without a single exception. And why are English authors to be adduced on the same subject by Dr. Jackson, and to be rejected when quoted by his antagonist? And let me ask too if there is any more, or even half so much difference, between the climates of Great Britain and the United States, as there is between that of the latter and the climate of the West-Indies? Surely there is much more propriety in quoting English authors in matters which relate to medicine among us, than there can be in adducing the writings of West-India practitioners. I shall, therefore, not hesitate to bring quotations against the doctrines of Dr. Jackson, from Willan, Bateman, and all other authorities, although they may have acquired their experience in England or the West-Indies.

Sir John Pringle is full and pointed in his testimony upon the prevalence of typhus in hot weather.

In his account of the campaign in Germany in 1743, he tells us of the prevalence of the hospital or jail-fever in July and August, during the existence of great heat.* In the history of the campaign in Britain he speaks of the fever's occurring in May

* Observations on the Diseases of the Army. Lond. edit. of 1752. p. 27.

when the weather was unusually dry and warm;* and also of its existence in the month of August of the year 1746.† And he lays it down as a matter of universal experience, that “the hospitals of an army, when crowded with sick, &c. especially in hot and dry weather, produce a fever of a malignant kind,” &c.‡

The Oxford Assizes, so memorable in medical history for the dreadful malignant fever (certainly typhus gravior, or jail-fever) which succeeded the sitting of the court, were held in July: and the sessions at the Old-Bailey, to which Sir John Pringle refers, took place in May.§

Dr. William Hillary, in his “Observations on the Changes of the Air, &c. in Barbadoes,”|| gives an account of a slow continued nervous fever, (known to be typhus from the description he gives of it,) which prevailed in February, although the thermometer had not been less than 74° at any time during the month.

Dr. Lind in his valuable Essay on the Diseases of Hot Climates, gives us an extract from the journal of a surgeon of a ship which sailed up the rivers of Guinea, and whom of course he must have respected as good authority. “In the month of June, almost two thirds of the white people were taken ill. Their sickness, &c. approached nearest to what is called a nervous fever.”¶ And in another chapter he says upon his own authority: “A near approach to such putrid swamps, is apt to produce an immediate sickness, a vomiting, and afterwards a low nervous fever.”**. This he says immediately after telling us of the presence of “intolerable scorching heat.”

Dr. Ferriar, in the history of an epidemic fever (evidently typhus) which prevailed in Manchester in 1789–90, says: “Several patients came under my care, during the intense heat of the last summer.” And again: “All these cases occurred, during the excessive hot weather of July and August, 1791.†† In another chapter the same author remarks: “But in the summer and

* *Observ. on the Diseases of the Army.* Lond. edit. of 1752. p. 56, 57, 58.

† *Ibid.* p. 64.

‡ p. 291.

§ p. 345–6.

|| Lond. edit. of 1766. p. 36.

¶ First Am. edit. p. 49.

** *Ibid.* p. 108.

†† *Medical Histories and Reflections*, first American edit. p. 72, 74.

autumn of 1794, the usual epidemic fever [typhus] became very prevalent among the poor, &c." p. 230.

Huxham enumerates warm weather among the causes of slow or nervous fevers. "This is the case in what we call slow or nervous fevers; which are generated by — warm and wet seasons, &c."* In his works† we find this striking passage: "It is certain from the observations both of the ancient and modern physicians that putrid, slow fevers rage most in a too moist and thick constitution of the air, continuing a considerable time, particularly if unusual heats accompany it."

Dr. Haygarth has so many observations upon the point I am endeavouring to establish, that it would be quite sufficient to refer the reader, in general, to his "Letter on the Prevention of Infectious Fevers." Yet it may not be improper to point to p. 12, where he speaks of typhus beginning in May, and extending epidemically through June, July, August, and September. The tables with which he furnishes us to illustrate the progress of this fever, (p. 20, 22, 24, 26,) exhibit its steady march through families in the summer season.

Dr. Wilson Philip has the following observation. "It is remarked above, that there is no particular state of the weather, which, independently of the circumstances pointed out as the sources of typhus, are capable of producing this fever."‡ Now in the enumeration of the sources of typhus, he makes no mention whatever of heat or cold.

Dr. Thomas, in his chapter on the typhus mitior, observes that "a season of continued heat, and particularly when combined with moisture, appears also to predispose the human body to receive the impression of contagion." Modern Practice, 3d American edit. p. 41. And elsewhere he says, "A close state of the atmosphere, with damp weather, is likewise apt to give rise to typhus gravior." p. 50.

Dr. Willan, in his valuable little book on the diseases of London, describes typhus as prevailing in that city in August and

* Huxham's Essay on Fever. Lond. 1782. p. 16.

† Huxham's Works, Vol. I. p. 180.

‡ Wilson on Fevers, Vol. I. p. 157.

September; and so far from considering cold weather essential to its extension, says that "its progress is generally stopped by the frosts of December." p. 43.

Dr. Bateman declares that "at all times there appears to be a greater disposition to fever in London during the autumnal months, which diminishes with the approach of winter. The present epidemic, [typhus,] if we may deduce such an inference from the demands upon the house of recovery, was most particularly prevalent from the beginning of August to the middle of November. The monthly admissions were in June 28, July 22, August 67, September 81, October 109, November 92, December 68." On the Epidemic, &c. prevailing in London, 1818, p. 16.

The testimony afforded by Dr. Edward Percival is so positive in favour of the fact for which I am contending, that I am really surprised at Dr. Jackson's quoting him as authority in his own favour. One of the very passages adduced by Dr. Jackson, though a *garbled* extract from Dr. Percival's excellent treatise, is a decisive refutation of his unwarranted notion. "An opinion has sometimes been hazarded, that the severer form of typhus is extinguished by the heats of summer; and this appears to have been the frequent experience of our armies and navy in warm climates. *But the fact is otherwise, not only in the cities, but in the country parts, of our island. Examples of the worst kind of typhus have occurred to my observation throughout the summer.*"* In another place he observes: "The mere vicissitudes of temperature, in the heats of summer and the cold of winter, have manifestly failed even to moderate the pestilence." And immediately below he tells us that the distemper "has baffled every vicissitude of weather, throughout a year of extraordinary temperatures."† And in p. 128, he speaks of "*the typhous epidemic of summer.*"

As Dr. Jackson has triumphantly referred to the records of the fever hospitals of Great Britain as presenting an irresistible mass of testimony in his favour; it may be proper to examine some of these records, and to see whether they really do award to him the victory he so boldly claims. He has quoted a table from Dr.

* Practical Observations on Typhous Fever, p. 39.

† Ibid. p. 6.

Currie, showing that fewer fevers were admitted into the Liverpool Dispensary in August than in any other month. It is evident that Dr. Jackson has here, as in many other places, altogether departed from the point in dispute. It can avail nothing in support of his assertion that "typhus never prevails in hot weather," to show that it is *more prevalent* in the winter. But to let the reader see how apt Dr. Jackson is to make a sweeping inference from a hasty investigation, or a solitary fact, I extract from Dr. Rogan's Observations on the Epidemic of Ireland, the following table. See p. 31.

Return of patients entered upon the books of the Strabane Dispensary from January 1817, to December 1818.

| | 1817. | 1818. | | 1817. | 1818. |
|-----------|-------|-------|------------|-------|-------|
| January, | 9 | 83 | July, | 60 | 106 |
| February, | 13 | 46 | August, | 206 | 90 |
| March, | 6 | 60 | September, | 287 | 57 |
| April, | 13 | 48 | October | 233 | 49 |
| May, | 3 | 39 | November, | 193 | 40 |
| June, | 10 | 71 | December, | 140 | 30 |

Upon calculation the reader will discover that out of 1892, the whole number of fevers received, no less than 887 occurred during the *four* summer months, June, July, August, and September. This will make the proportion of cases occurring in summer (only a third of the year) *nearly one half*.

It is needless to occupy more space upon this point; but I must request the reader to examine the tables published by Dr. Haygarth in his Essay on the Prevention of Infectious Fevers,* for evidence that typhus fever is not necessarily restricted to the winter season, but will frequently prevail most extensively in the hottest months of summer.

Having clearly refuted Dr. Jackson by the foreign authorities which have been advanced, I shall now bring forward a few American writers whose testimony on the prevalence of typhus in summer, is equally conclusive. Whatever objections Dr. J. may invent against English authorities, I presume he will have no hesitation in receiving the evidence of our own writers.

* Page 20.

The first author I shall adduce is Dr. Gallup. And perhaps I cannot do better than quote the passage already adduced by the reviewer. "This disease prevails most frequently in August and September; but no month is free from it, especially the forepart of the cold season."* But Dr. Jackson objects to this passage, that the typhus here spoken of is nothing more than synocha. Dr. Gallup himself, however, declares that the typhus mitior of which he is here speaking, is the same as that described by the English writers under that name. He says "he can discover no difference between them," p. 363. And indeed his account of the disease is sufficient to convince any body that they are identical. But this is not all the testimony afforded by Dr. Gallup on this point. He speaks in this same chapter, p. 364, of this disease degenerating into typhus gravior through neglect. He tells us too in p. 358, that during the prevalence of yellow-fever in Philadelphia from 1797 to 1803, typhus mitior appeared in the part of the country in which he resides. In p. 41, he says that "common fevers," (by which I think it is evident from the context he means typhus) "prevailed mostly in the warm season of the year." I would tire the patience of the reader, were I to exhibit all the passages which are to be found in Dr. Gallup's work, and which determine the question, so far as his authority is concerned. I must, therefore, content myself with referring to pages 43, 44, 45, 46, 47, 50, 52, 62, for more ample proof of the frequent prevalence of typhus fevers in the summer season.

In the Appendix to Dr. Hosack's Observations on Contagion, there is an account by Dr. Francis of a typhus fever which prevailed in the New-York Bridewell in the July of 1814, p. 76. Dr. Jackson may perhaps be disposed to question the lawfulness of this testimony, because it is published to the world through the medium of *Dr. Hosack's* book. But I hardly fear the reader will be so unreasonable. And here I must again take occasion to remark, that the attempts which have been made to represent those who have opposed the committee of the Medical Society in the Bancker's street affair, as *leagu*ing with the resident physician of the city of New-York to support his favourite theory of

* Gallup's Sketches, &c. p. 363.

the contagiousness of yellow-fever, are extremely illiberal and unjust. For my own part I disclaim all participation in any such league: and as for the reviewer, whom Dr. Jackson has been pleased to designate as Dr. Hosack's *coadjutor*, I know that he is not an advocate of the system referred to.

In the *New-England Journal of Medicine, &c.* Vol. IV. p. 228, will be found the history of a typhus fever which prevailed in Wardsborough, Vermont, in the summer and autumn of 1814, by Dr. Allen. And Vol. VII. of the same work contains an account of a typhus fever which appeared in the Boston Alms-house in 1817 and 18.

In the *American Medical and Philosophical Register*, Vol. II. p. 9, is the account, by Dr. D. R. Arnell, of a fever, evidently typhus, which occurred in Orange County, N. Y. in the August of 1809. And in the same work, Vol. IV. p. 36, is to be found a paper by the late highly respectable Dr. M'Bride, in which is described a typhus gravior which prevailed epidemically in one of the districts of South Carolina in the summer of 1808. 'Tis true, the *Register* was conducted by *Dr. Hosack*, and by quoting it I may subject myself to the imputation of being one of his *coadjutors*; yet I presume that the testimony of Dr. Arnell and of Dr. M'Bride will not be rejected by the reader, however unpalatable it may be to Dr. Jackson.

In the number of the *Recorder* for January last, p. 83, Dr. Joseph Klapp describes a disease exhibiting the different grades of typhus, as prevailing in the summer of 1820 in the Philadelphia Alms-house.

I come now to an authority which will no doubt be received with some deference by Dr. Jackson, as the work was set up for the avowed purpose of vindicating the domestic origin of yellow-fever, and has always been conducted in the exclusive and intolerant spirit which characterizes the majority of the party who espouse this doctrine. Yes, even the *Medical Repository* will condemn this champion of the cause of non-contagion. In the XVth volume of that work, p. 223, Dr. Peter C. Tappen describes several cases of typhus fever as occurring in the N. Y. Hospital in midsummer of the year 1811. Nor can this testi-

mony be evaded by saying, that it rests merely upon the private opinion of the gentleman who communicated the cases. The paper was drawn out at the request of one of the editors, who was himself the attending physician in the hospital at the time this fever made its appearance. But this is not all which the Repository speaks on the point in question. In a communication upon the diseases in the vicinity of the Roanoke river in North Carolina, we are expressly told of typhus prevailing epidemically in the spring of 1799, and in the summer of 1806. And indeed the writer intimates that this is the case in every dry season. Vol. XI. p. 339. We learn again from the same journal, that at Wilmington (Del.) a prevailing dysentery was transformed into an epidemic typhus in the August of 1803; and that this fever raged with unusual violence. Vol. VIII. p. 142.

I shall protract this examination of Dr. Jackson's "remarks" to introduce only one more authority. In the second number of the Philadelphia Journal of Medicine, p. 444, Dr. Chapman, the Editor, in speaking of the Bancker's street disease, says he thinks it probable that it was, *as is alleged by Dr. Hosack*, of a *typhoid* character, such as existed in Philadelphia at the same time. Now it is well known that Dr. Hosack has always maintained that the fever of Bancker's street was *typhus*. We have, therefore, the concurrent testimony of Dr. Chapman to the same effect.

Dr. Jackson, I am aware, affects to make a difference between the terms *typhus* and *typhoid*. He contends, that as the latter is compounded of *τυφος* and *ειδος*, it can be applied only to diseases presenting the prominent symptoms of, or bearing a *resemblance* to, typhus fever. It is evident that this distinction is not made by Dr. Chapman in the passage referred to; and I am bold to affirm, that no author of established repute in medicine recognises the difference contended for by Dr. Jackson. The terms are absolutely *synonymous*. But let us inquire whether the etymology of the word *typhoid*, will justify this assertion. *Ειδος* signifies *facies, species, forma*; and the compound which it forms with *typhus*, can signify nothing but a disease bearing the likeness, possessing the nature, and exhibiting the form of, typhus itself. I grant, indeed, that when used by medical writers, it is seldom

intended to designate typhus fever itself, but rather some disease in which the fever is of a typhous character. But it may with propriety be used synonymously with the word typhus, and in common parlance almost always is. I cannot, therefore, allow Dr. Jackson to screen himself by this quibble from the inconsistency charged upon him by the reviewer. He states in one place, that a "fever of a bilious and remittent character, combined with *typhoid* symptoms," existed in Philadelphia last summer; and in another, he denies that typhus ever occurs in hot weather. If this is not an inconsistency in fact, it is, to say the least, a contradiction in terms. Nor can I allow him to plead the benefit of this quibble for the unfortunate committee whose destitute cause he has espoused. For they positively assert, that "during the months of July, August, and September, and the early part of October, there occurred at the Bellevue hospital, [not a *typhoid* disease, but] a disease exhibiting many *characteristics* of the typhus gravior, jail or hospital fever." See Report, p. 11.

A remark or two more, and I have done. I confess myself wholly at a loss to find a motive for Dr. Jackson's voluntary defence of the report, unless upon the principles before suggested. He certainly does not believe that the committee are *correct* in calling the Bancker's street disease yellow-fever; for, in his "remarks," he expressly says that it was a "*bilious* fever displaying *typhoid* symptoms." See last No. of the Recorder, p. 435. Why then assist the committee in the defence and propagation of an error? But consistency is hardly to be expected from a man who, at one time, makes a most merciless and rude attack upon the Board of Health of New-York, for concealing the existence of yellow-fever in that city;* and at another, comes out and plainly says that the disease was nothing more than *bilious* fever with typhoid symptoms.

After this review of Dr. Jackson's "remarks," the reader will perceive that he has completely *mistaken* the point of contro-

* I must beg the reader to look at Dr. Jackson's paper in the second number of Dr. Chapman's Journal. I am afraid he will hardly believe me, unless convinced by *seeing* it himself.

versy, and may justly be upbraided in the language of Cicero: "Utitur in re non dubia testibus non necessariis." He will perceive too, that the prevalence of typhus in summer is triumphantly proved to be possible, notwithstanding Dr. Jackson's declaration that "*all* experience and *all* authority" prove that it *never* is. And, in short, he will be prepared to pronounce Dr. Jackson's attack upon the reviewer a complete *abortion*.

In conclusion, I offer a few remarks in defence of the style and spirit of the review complained of. It must be confessed, that it is written with a severity which may bear the construction of harshness. Yet I really cannot perceive in it the abusive personalities it is said to contain. I can well believe, however, that it is calculated to excite very unpleasant feelings in the breasts of those against whom it is directed; and that it may be construed by them in such way as to speak things which were never *designed*. The faculty of *association* is wonderfully acute and sensitive. But I see in it the breathings of a generous and lofty indignation, mingled indeed with sarcasm and humour, but always preserving a becoming dignity. Had Dr. Jackson witnessed the public discussion to which this report gave rise in the Medical Society of New-York, or had he seen it in the form in which it was presented to that body by their committee; he would not, I am confident, have censured any degree of severity with which it might be treated, or accused the reviewer of indecorous levity and disrespect. The fact is, the report was written in so malignant and diabolical a temper, that the society would not receive it in its original form. And the committee exhibited, in defending it, so much malicious pertinacity, that it was not, until after a protracted discussion, that the friends of moderation and decorum could succeed in modifying it into the less obnoxious shape in which it now appears. Will it be wondered at, then, that the committee should be handled with severity? Or will it be a matter of surprise, that one of the very individuals who were designated as the victims of their malice, should be the instrument of inflicting the chastisement they deserved?

Dr. J. has kindly given the reviewer a piece of *advice* by way of conclusion to his "remarks." He will not, therefore, be

offended if I suggest to him a caution more apposite perhaps than the one he has offered the reviewer; and which if observed will, I am sure, be useful.

“ Neu, cave, defendas, quamvis mordebere dictis.

“ Causa patrocinio non bona *pejor erit.*

Ovid. Trist. Eleg. 1. Lib. 1.

D.

[I think it right to state, that I disavow any participation in the personal allusions, contained in the foregoing article, furnished by my co-editor at New-York,

J. EBERLE.]

REVIEWS.

Quidquid venerit obvium, loquamur
Morsa sine cogitatione.

MARTIAL.

ART. XII. *A Treatise on Inflammation of the Mucous Membrane of the Lungs. To which is prefixed, an experimental Inquiry respecting the contractile Power of the Blood Vessels, and the Nature of Inflammation.*—By CHARLES HASTINGS, M. D. Physician to the Worcester Infirmary; late President of the Royal Medical Society Edinburgh. 8vo. pp. 420. London, 1820.

THERE are no diseases more common, or more generally fatal, than those of the pulmonary organs. Exposed as the lungs are to the direct influence of atmospheric impressions, and holding powerful sympathies with other parts of the system, the diseases to which they are liable form a large proportion of those which fall under the attention of the medical practitioner. The mucous membrane lining the trachea and bronchial tubes, is the part most generally affected in the acute diseases of the lungs. Until within late, however, the different tissues of the pulmonic organs have not been properly discriminated by authors in their accounts of the diseases of these parts. They have treated of "inflammation of the serous, cellular, and mucous membranes of the lungs under one common title—pneumonia." Bordeu, Pinel, and particularly Bichat, have furnished us with many important observations on this subject. They have shown by their anatomico-physiological researches, that the animal body is made up of distinct systems of structure; each possessing a peculiar modifi-

cation of the laws of vital action, and consequently exhibiting peculiar modifications of the general phenomena of inflammation. They have also made us acquainted with the important pathological fact, that the inflammation of one particular tissue is not readily communicated to those which are adjacent.

Bordeu appears to have been the first who has treated expressly on the diseases of the mucous membranes of the lungs.* Since his time, various writers have published detached observations on the inflammatory affections of this membrane; but until the work of Badham† appeared, we had no treatise which was devoted entirely to inflammation of the mucous membrane of the bronchiæ. Notwithstanding the excellent character of this book, it does not, we conceive, do away the necessity for such a work as the one we are now reviewing, which exhibits a more precise and extensive view of the subject.

Preliminary to the more immediate object of his work, the author considers, at considerable length, the question relative to the contractility of the arteries, and the pathology of inflammation in general. Having fixed the precise ideas which he attaches to the terms employed to express the vital properties of the animal system, he proceeds to examine what agency these properties have in carrying on the circulation of the blood. Upon this point a great deal of controversy has existed, and, indeed, does still exist. Some writers contend, that the action of the heart is adequate, without the concurrence of any other powers, to circulate the blood; whilst others are of opinion, that the blood-vessels are endowed with an inherent irritability, by which they contract on the blood, and propel it forwards in its course. Dr. Hastings does not agree with those who adopt the former of these two doctrines: his arguments, however, in favour of the latter opinion, although ingenious, and possessing considerable weight, are not sufficient, we conceive, to establish his sentiments upon a solid basis.

Dr. Wilson Philip and Dr. Carson suppose that the dilatation of the chambers of the heart follows their contraction, and thus

* *Tissu muqueux, ou l'Organe Cellulaire, et sur quelques Maladies de la Poitrine*, par Theophilus Bordeu. 1790.

† *Badham on Bronchitis*. London. 1808.

forming a tendency to a vacuum, suck in the blood from the veins, Plausible as this doctrine is, there are, notwithstanding, facts at hand which go directly against it. "If (says Dr. Hastings) a ligature be applied to a vein, the blood accumulates, which should not happen if the tendency to a vacuum at the heart gave rise to the circulation in the veins; because the gravity of the blood, according to Dr. Carson's view, should direct it onwards through the numerous anastomosing vessels towards the auricle, where all atmospheric pressure during its diastole is removed."

The author then passes on to consider the question relative to the contractility of the blood-vessels. He decides in favour of the existence of such a power, and enters at large into an examination of the arguments, both *pro* and *con.* that have been advanced upon this subject.

"Of all our predecessors Vershuir is the most accurate on this point. His thesis contains most unanswerable results. He relates twenty-one experiments, which he performed on the larger arteries of dogs and other animals. In all of them, after dissecting away the integuments covering the arteries, he irritated the vessels with a scalpel, and observed whether any contraction ensued. In four he could not detect indications of irritability, although he applied various stimuli to the arteries.* In six instances the effect was not sufficiently evident for him to determine whether it should be attributed to the irritable property or to some other cause. His mind, therefore, remained doubtful for some time; but when, from other more decisive experiments, in which every caution to avoid error had been employed, the most satisfactory proofs of irritability appeared, he ceased to think the result of the six experiments at all doubtful. He adds, that these experiments were witnessed by several friends; and in one instance the effect of the stimulus was so manifest, that the most sceptical must have been convinced of the existence of an irritable property in the arteries." p. 16.

The experiments of Mr. John Hunter and Dr. Jones go to prove the same points. The author gives a tabular view of

* Vershuir de Arteriarum et Venarum vi irritabili. Exp. 12, 15, 20, 21.

twenty-five experiments, which argue strongly in favour of the irritability of the blood vessels. In his observations upon the results of these experiments, he makes many very interesting and judicious remarks. It is well known that Dr. Parry is a strenuous advocate for the exclusive agency of the heart, in sustaining the circulation. This writer also denies the contraction of the arteries during the systole, and maintains, that the pulse is caused solely by the resistance made by the blood against the finger, or whatever else diminishes the canal along which the blood is moving with increased impetuosity; when the left side of the heart contracts. These views are ably, and we think satisfactorily, combated by our author.

“The facts (says he) already adduced, most unequivocally demonstrate the arterial tubes to possess a high degree of vital contractility. But their active agency is not only supported by such experiments as those related; it is also countenanced by an extensive series of phenomena presented during disease in the human subject. Of these, may be mentioned irregular determinations of blood, the growth of tumours, increased pulsation of the arteries leading to inflamed parts, of which the following is a well marked example, the accuracy of which may be entirely relied upon. The carotids, when the person alluded to is in health, beat equally as to strength and frequency; but when he is attacked with inflammation in the right tonsil, to which he is particularly subject, and which sometimes proceeds so far as nearly to prevent deglutition, each pulsation of the artery gives a throbbing sensation on the right side of the head. On the application of a hand at this time to each carotid, the right is found to beat much stronger and fuller than the left.” p. 32.

Having noticed the objections that may be urged against the evidence adduced in favour of the irritability of the arteries, our author proceeds to consider “the phenomena which may be observed in the capillary vessels.” Whatever may be thought of the contractility of the larger vessels, the phenomena that belong to the capillary system prove beyond a doubt, we think, that *they* are endowed with the principle of contractility in no small degree. It seems indeed quite unaccountable, that Haller, who did allow

some degree of irritability to the larger arteries, should have denied this property to the capillaries. The phenomena of blushing, the supervention of local inflammation, are, we think, sufficient to settle this point. Whytt saw these objections to the doctrines of his illustrious antagonist, and he adduced in addition to these, the fact of the more copious flow of saliva in hungry persons; "the flow of tears by acrid applications to the eyes;" all of which phenomena could never appear from the action of the heart alone.

The author next exhibits a tabular statement of 43 experiments, with the microscope, on the blood vessels of the web of a frog's foot, the general result of which go to prove:

"1. That the application of a stimulus often quickens the circulation in the small vessels, whilst the motion of the blood in a neighbouring part, to which no stimulus is applied, remains unaffected."

"2. That when a small vein or artery is touched by a stimulating substance, a contraction is often produced, so as to be visible by the help of the microscope. This contraction sometimes proceeds to such an extent as to prevent the free passage of the blood; and in this case it does not accumulate, but takes a retrograde course. During such contraction, if not sufficient to prevent the transit of blood, the impulse of the ventricle is generally perceptible in all the vessels in communication with the contracted one, and the blood in the capillaries connected with the contracted vessel, for the most part, moves slower."

"3. That a stimulus in the first instance often produces a quickened motion of the blood, and contraction of the vessels; but after it has been applied some time, dilatation of the vessels, and a slower movement of the blood follow; but after the vessels are dilated by the action of one stimulus, some other stimulus will often produce contraction."

"4. That the action of water, heated considerably above the temperature of the animal to which it is applied, often occasions contraction of the vessels and acceleration of the blood's motion; but after a certain time, dilatation and retarded circulation ensue. Ice generally produces a contraction of

these dilated vessels, and restores the velocity of the circulation."

"5. That ice, kept in contact with the web of a frog's foot, produces, in the first instance, a contraction of the capillaries, and increases the motion of the blood; but after a certain period, if the application be continued, the vessels become dilated, and the blood moves slowly."

Next follow some interesting experiments, intended to show that the veins are not devoid of an active contractility.

The author now proceeds to treat of the general nature of inflammation. He first reviews the various theories that have been published at different periods on this subject. Those of Boerhaave, Cullen, Hunter, and Vacca, are particularly noticed. He adopts that of VACCA.* This doctrine was first published at Florence in a book dated 1765. It was afterwards taken up and ably defended by Dr. Lubbock and Mr. Allen in the Medical Society of Edinburgh, about the year 1790. Dr. Wilson Philip was, however, the first who supported this doctrine by the direct evidence of experiment. He applied the powers of the microscope to ascertain the changes which take place in the vessels of an inflamed part. By the interesting experiments of this writer, it was shown that the vessels, so far from being in a state of increased action, are, on the contrary, passively distended, and the motion of the blood extremely languid. These results confirmed the opinions published by Vacca, Lubbock, and Allen, and led him to the cer-

* PROP. 1. Inflammatio cujusvis partis humani corporis nusquam fit, nisi in ipsâ parte sanguinis coarcevetur, et ferè quiescat.

PROP. 2. Coarcevatio et semistagnatio sanguinis vel alius humoris corporis humani in quâcunque ipsius corporis parte minimè contingere potest, sine ipsius partis absoluta vel relativa debilitate.

PROP. 3. Data eadem partis cujusdam debilitate, non solum coarcevatio, et semistagnatio sanguinis fiet, in ipsius partis sanguineis vasculis, ut demonstratum est, verum etiam canales laterales lymphaticos, et adiposos ipsius partis sanguis ingredi debet.

PROP. 4. Ex majori collectione sanguinis in vasculis sanguineis alicujus partis, et ex ingressu ipsius in canales tam lymphaticos quam adiposos et ex ejusdem sanguinis per ipsos atque sanguineos canales lentissimo motu inflammatio morbosa in eâdem parte osiri potest.—*Liber de Inflammationis Morbosæ quæ in Humano Corpore fit Naturâ, Causis, Effectibus et Curatione.*

tain conclusion, "that inflammation consists in debility of the capillary vessels, followed by an increased action of the larger arteries, which is terminated as soon as the capillaries are so far excited, and the larger arteries so far weakened by the preternatural action produced in them, that the power of the capillary vessels is again in due proportion to the *vis à tergo*." In the year 1813, professor Thompson, of Edinburgh, published several experiments, instituted for the purpose of ascertaining the nature of inflammation, and from which he draws conclusions opposed to those which Dr. Philip had drawn from a series of similar experiments. According to Dr. Thompson, "Inflammation is sometimes attended by an increased, and at others by a diminished velocity in the circulation, through the capillary vessels of an inflamed part, and consequently that neither of these states ought to be included in the definition which we give of inflammation."*

To reconcile this contrariety of opinions of two writers who rest their sentiments upon similar experiments, Dr. Hastings undertook a course of experiments on this subject, which appear to have been conducted with much ability and judgment. He relates nine experiments, the results of which seem to us very satisfactory. The conclusions which he draws from them are:

"It appears that certain stimuli applied to living parts produce an increased velocity of the blood's motion, and a contraction of the blood vessels. But if the stimulus be long continued or increased in power, the small vessels which, in the natural state, admit only one series of globules, become so dilated as to allow an accumulation of a much less fluid and redder blood in them, which loses its globular appearance, and moves much more slowly than that which previously passed through the vessels. The part now appears inflamed. If the stimulus be removed the vessels do not soon regain their original state; time is necessary to allow them to recover their contractile power, so as to prevent the impetus with which the blood is propelled by the heart and larger arteries from keeping up the dilated state of the capillaries."

* Lectures on Inflammation, by John Thompson, M. D. Edin. 1813.

"If the stimulus which produces the inflammation be of a very acrid nature, debility of the vessels is frequently induced without any previous excitement. The blood in all the smaller vessels becomes very red, circulates very slowly, and in some vessels stagnates."

"The application of a stimulus different from that which produced the inflammation will sometimes bring on resolution. When this occurs the dilated vessels contract; they no longer contain a red dense homogeneous fluid, but again receive blood, consisting of small, nearly colourless globules, which float in a colourless fluid, and the motion of these globules, at length, becomes as quick as before the inflammation took place. If, however, the inflammation proceed, the blood becomes nearly stagnant, it continues very red, and the vessels are much dilated." "The changes that take place in the appearances of the blood in an inflamed part are very important, but have not been particularly described by those who have treated of the state of the blood vessels in inflammation."

"In the author's experiments, the weakened action of the smaller vessels was always accompanied with an alteration in the appearance of the blood. In the natural state of this fluid, globules can be distinctly seen; but after inflammation has commenced, the globular structure disappears, the blood becomes redder, and the most minute capillaries are distended with it" p. 95.

These alterations in the blood may, perhaps, admit of different explanations. The author thinks that they depend on *chemical* changes. "The accumulation (he says) of the globules in the capillaries, may be so great, and the vitality of the vessels so much diminished, as no longer to resist the tendency of the constituent parts of the blood to enter into new combinations; and hence chemical changes ensue." Several experiments are given, which render it probable that a chemical change does take place in the blood of an inflamed part.

The difference between active and passive inflammation, according to our author, depends on the presence or absence of increased action of the larger arteries leading to an inflamed part.

“On this (says Dr. H.) in a great measure depends the difference between the ordinary case of acute inflammation of the tonsils, in which there is increased pulsation of the carotid, flushed face, throbbing of the head, excruciating torture in deglutition, with a hard and full pulse; and the less frequent, though gradual increase of size in the tonsil, pallid countenance, no pain or throbbing in the head, difficult, though not very painful deglutition, with a weak pulse. In both these instances the tonsils are inflamed, but the disease in the one case is very different in character from that in the other. The variety in the symptoms seems to arise from the degree of excitement in the larger trunks.”

After giving a very interesting account of the *ratio symptomatum* of inflammation, our author goes on to notice in the 2d chapter, “the opinions of preceding writers respecting bronchial inflammation.” We shall not, however, occupy the reader’s time by dwelling on this portion of the book; but proceed to give some account of the more immediate object of the work—*bronchitis*.

The author describes seven varieties of bronchial inflammation, which he divides into acute and chronic. He gives, in the first place, a general history of the symptoms, nature, and treatment of several varieties of each kind, and afterwards relates such cases as are calculated to elucidate the subject.

I. The first variety mentioned under the division of acute bronchites, is the common catarrh. This is the mildest form of the disease. That the mucous membrane of the trachea and bronchia is slightly inflamed in this variety, is quite probable, from the “frequent supervention of highly inflammatory symptoms, and also by the common degeneration of this disease into an obstinate chronic affection in persons of delicate habits, when neglected.” The cough then becomes very severe, the expectoration copious, and resembling pus; the dyspnoea considerable, the pulse quick, and all the usual symptoms of confirmed phthisis supervene. In such cases, tubercles are generally found in the lungs on dissection, and the mucous membrane inflamed and ulcerated, and the bronchia and air-cells filled with purulent matter.

II. This variety occurs most frequently in old people, or those of a phlegmatic habit, during sudden changes of the weather. It

corresponds with Dr. Sydenham's peripneumonia notha. The apparent mildness of the attack is apt to deceive both the patient and the physician. The fever is not violent; there is no fixed pain in the chest, but a sense of uneasiness and weight is felt across the breast. The respiration is quick and laborious, and generally attended from the beginning with much wheezing noise; and as the disease advances, the breathing becomes noisy and rattling, from the accumulation of the secretions in the air-cells and bronchia. Hoarseness is a very common symptom, and the patient cannot draw a deep inspiration with freedom, or without exciting a good deal of cough, or increasing pain. At first the dyspnœa is not increased by lying down, but in the course of the disease the respiration is much freer in the erect position. It is remarked by Dr. Badham, that in some cases of asthenic bronchitis the dyspnœa acquires periodical exacerbations resembling transitory fits of asthma, with sudden constriction across the thorax and momentary loss of voice. Cough is one of the earliest symptoms, at first often dry, but in the progress of the complaint some expectoration generally attends. If a copious expectoration comes on, consisting of thick, viscid, opaque mucus, the violence of the cough is usually much lessened. There is sometimes discharged a dense white mucus, moulded into the form of the ramifications of the bronchiæ. There is almost always great pain felt across the forehead, which is excessively aggravated by coughing. At first the fulness and frequency of the pulse is not much increased, but in the course of the disease it becomes both full and hard; never, however, acquiring the hardness of the pulse in pleurisy. Evening paroxysms of restlessness, with flushing of the face, come on. The blood is commonly buffed. In some cases the disease terminates in a few days.

“ In the more violent examples, when the remedies employed do not check the progress of the symptoms, the pulse, towards the seventh or eighth day, becomes very quick and much weaker; occasional perspirations break out; the nails and lips assume a slightly livid hue, and the countenance is distressed, anxious, and pallid, with somewhat of a purple tinge. In fact, every symp-

tom bespeaks obstruction in the air passages. Soon after the extremities grow cold, and the patient dies of suffocation." p. 163.

In cases less violent the disease begins to subside about the sixth or seventh day, by a copious expectoration of a bland mucus. In such cases, any sudden unfavourable changes of the weather will sometimes cause the disease to run into an obstinate form.

"A train of symptoms occasionally appears, very much resembling those of phthisis pulmonalis, and sometimes patients die under these circumstances with every appearance of tubercular consumption; dissection showing a slightly diseased state of the bronchial membrane, while the structure of the lungs is not affected."

III. This variety takes place in strong and plethoric habits, and the reaction is always violent. A cough almost constantly attends, and the dyspnoea is very great, the patient only experiencing tolerable ease in an erect position. There is rarely any fixed pain in the breast; but a distressing sense of constriction is constantly felt. There is some expectoration, a diminution of which is always a bad symptom. The skin is dry, the tongue foul, and the urine high coloured and scarce; pulse hard. This stage of violent reaction, unless speedily overcome by an appropriate treatment, generally terminates in a corresponding collapse of the system. When this takes place the orthopnoea is increased, the lips are purple, the pulse sunken and quick, the surface of the body cold and covered with perspiration. The vigorous measures usually employed in the commencement to check the dangerous symptoms, necessarily induce great debility, which commonly renders recovery slow, and too often brings on a tedious chronic disease, characterized by frequent and violent cough, attended with a copious puruloid expectoration, "which often carries the patient off in a state of marasmus and hectic fever."

IV. This variety is an acute affection, to which young children are peculiarly subject. It is even more speedily fatal than the last variety, but is not attended with symptoms of corresponding severity. It is an extremely deceitful disease, and death is often close at hand when a favourable termination is confidently anticipated. It begins in the shape of a catarrhal affection, and

generally retains this character throughout its whole course. The breathing is attended with a wheezing noise, but is not remarkably difficult. The patient remains easy in a horizontal posture. A slight cough always attends; tongue often loaded. The child rarely refuses sustenance, and the fever is often inconsiderable. The pallidity of the face is very remarkable. Remissions and exacerbations commonly succeed each other in an extraordinary manner in the state of the pulse and respiration.

“The breathing for several hours occasionally appears so free and easy, as to lead the practitioner to expect that he has subdued the disease, and the child will recover; when, suddenly, a great aggravation of the difficulty of breathing takes place, so as sometimes to threaten immediate suffocation; but by degrees the urgency of the dyspnœa subsides, and the breathing apparently becomes in some instances even nearly natural, and remains so till another aggravation of the symptoms occurs, which may not happen for a considerable time. During the remissions the child often doses, and is not much distressed by the cough. The disease is not stationary; the symptoms gradually become more distressing. The aggravations of the difficulty of breathing are more alarming, and the remissions less perfect.”

170.

Coma ensues; the lips and face become livid, and death soon takes place, commonly between the fourth and seventh days.

V. Under this variety our ingenious author treats of bronchitis as connected with various cutaneous diseases, viz. rubeola, variola, erysipelas, and that variety which supervenes after the sudden disappearance of chronic eruptions.

VI. Bronchitis, complicated with diseases of the abdominal viscera. The liver is most frequently implicated in cases of this kind. The bronchitis, in such instances, commonly assumes the form of the second variety.

“It is by no means uncommon to find the more severe bronchial inflammations accompanied by those symptoms which denote acute hepatitis. Pain is felt in the region of the liver, with a sense of tension of that part; the hypochondrium is tender on pressure, and the skin and urine are sometimes tinged with yel-

low. The mouth is dry, and the tongue is covered with a yellowish crust. The bowels are irregular, in general constipated, but occasionally a diarrhoea attends, and the dejections are dark and foetid. In no cases does this combination of acute hepatitis and bronchitis more commonly occur, than when inflammation of the mucous membrane of the lungs succeeds rubeola." 178.

VII. Bronchitis existing with other diseases of the respiratory organs. Of these, the most common is chronic inflammation of the larynx and upper part of the trachea, and sometimes ulceration of these parts.

"The symptoms which denote the first attack of chronic inflammation of the trachea, which is very insidious in its approach, are, a tickling cough and tenderness in some parts of the trachea, pressure generally producing a cough. The voice is always changed, the patient often speaking in hoarse whispers. The pulse is accelerated, fever for the most part attends, and a dense mucus is often expectorated.

"If the disease proceed to ulceration, the voice becomes more affected, the pulse generally accelerated, and the cough is harassing, attended with a copious purulent expectoration. The countenance expresses great anxiety, and the respiration is laborious; but no pain is felt in any part of the chest, though it is often referred to the epigastrium, when the dyspnoea has continued long." 179.

If the patient, while suffering under this chronic disease, is exposed to cold, or any other cause that may bring on a general inflammatory condition, an acute inflammation may be excited in the affected parts, which diffuses itself over the whole of the mucous membrane; and thus the symptoms of bronchitis are superadded to those of chronic disease of the trachea; the straitness across the breast becomes more distressing, the breathing more difficult, the lips and face become livid, and the fever is augmented.

There is no form of bronchitis which is attended with more distressing or fatal consequences than that which arises from tumours pressing on the wind-pipe

Appearances on dissection. The appearances on dissection are various, according to the antecedent symptoms. In the severer

cases, the lungs do not collapse when the thorax is opened, although the pleuræ do not adhere, and the parenchymatous structure of the lungs be sound. When the trachea is laid open, it is "often found full of fluid, which is sometimes purulent, and at others consists of serous matter, with which coagulable lymph, or mucus, is often mixed. The bronchia are for the most part plugged up by purulent matter, or tenaceous, mucous, or bloody serum. When the lungs are cut into, a frothy fluid escapes. In a few instances, no fluid is found in the trachea, bronchia, or cells, although strong marks of inflammation exist." 183.

The structure of the lungs in acute bronchitis, though not commonly, is nevertheless sometimes affected with inflammation. The substance of this viscus is then either reddened, hardened, suppurated, or tuberculated. In chronic bronchitis such appearances are still more common. The pleura, too, is often inflamed, exhibiting a whitish incrustation, or an effusion into the cavities of the breast. The stomach is sometimes inflamed, and even ulcerated. The peritoneum is also sometimes found in morbid condition.

"Its whole surface is granulated; the intestines are united together and covered with a white incrustation, and a milky, puriform fluid is contained in its cavity."

We shall pass over the section "*On the nature of the foregoing affections of the bronchia;*" and proceed to give some account of the *treatment* of these affections, as given in the excellent book before us.

"The following," says Dr. Hastings, "is a concise view of the means we have recourse to for removing these inflammatory affections. To moderate the excitement of the sanguiferous system—general blood-letting, acidulated and mucilaginous drinks, and abstinence from all stimulating food. To promote expectoration and perspiration—antimonial and saline medicines. To direct the fluids towards the surface, and relieve the congestion of the debilitated capillaries—local blood-letting, blisters, and rubefacients."

Our author observes further, that blood-letting is by far the most powerful remedy for diminishing the excitement of the sys-

tem. It cannot, however, be applied in all the varieties of bronchitis. In the *first* variety blood-letting is seldom necessary, unless the febrile symptoms be considerable, when it may be employed with advantage. In the *second*, bleeding is generally proper, but should not be rashly employed.

“ The abstraction of ten ounces of blood from the arm early in the disease, sometimes mitigates the symptoms ; after which it is generally more safe to depend on an attention to diet, proper expectorants, and local evacuations. The peculiar tendency to effusion, often renders the treatment of this affection difficult, as we are sometimes deterred by this cause from pursuing the blood-letting, when the inflammatory symptoms indicate its employment.” 209.

When bronchitis occurs, as in the third variety, in robust habits, and the symptoms of reaction run high, it soon terminates in irremediable debility. The time which is allotted for the employment of depletory measures, is therefore short, and should be promptly turned to advantage. “ With this view, blood-letting should be boldly employed. From twenty to thirty ounces of blood may be taken from the arm in severe cases, at the first blood-letting. If the patient be of a strong habit, and the pulmonary symptoms urgent, and the febrile excitement considerable, we should allow it to flow till the pulse becomes weak, or the pulmonary symptoms are relieved.” The bleeding must generally be repeated, before the disease yields.

In young children, where it is sometimes difficult to obtain blood from the arm, much advantage is derived from taking it from the jugular vein. Although children do not, for the most part, bear the loss of blood well, yet, when life is menaced, we have no alternative; blood must be drawn, and that in no small quantity, or the disease will almost certainly prove fatal: but even this vigorous practice will not always suffice to arrest the progress of the disease.

“ When bronchitis is complicated with chronic disease of the trachea, blood-letting to any great extent cannot be employed; for, if ulceration of the trachea have come on, the strength is

often so much exhausted by it, that the loss of any considerable quantity of blood is not admissible."

In the second variety, vomiting has been very highly spoken of by several writers. "The benefits which arise from vomiting are twofold; they unload the primæ viæ, thus removing the causes of irritation, and they increase the expectoration," on which the favourable issue of the case so much depends. Stimulating expectorants, says Dr. H., cannot be proper in the second variety, if the heat be much increased. When, however, the inflammation is nearly subdued, and the bronchia are loaded, our author recommends a combination of squills and ipecacuanha as very serviceable. He does not think much of the inhalation of watery vapours. Cathartics are not generally regarded as suitable remedies in thoracic diseases. Dr. Hastings, however, advises that the body should be kept lax, in every variety of bronchitis; in the beginning of the disease, the alimentary canal should be cleared by an active purgative.

Where bronchitis is combined with abdominal disease, our author thinks the free employment of cathartics of the utmost importance. When the liver is affected, cooling and mercurial purgatives must be used; yet mercury, when it acts upon the general system, does not appear to be beneficial, except when, in the sixth variety, "the liver is evidently affected, small doses of blue pill may be given, so that the gums may become tender."

In bronchitis, combined with trachitis, attended with symptoms resembling croup, Dr. Hastings recommends the employment of calomel in frequently repeated doses. He mentions one case, where bronchitis occurred in combination with chronic inflammation of the mucous membrane of the trachea, in which the exhibition of calomel in this way afforded great advantage. When the fever declines, and there is still a considerable irritability of the system and air passages present, opium is often of great service in allaying the cough and calming the patient.

"In combination with small doses of calomel, opium may sometimes be exhibited at an earlier period of the disease. When conjoined, these remedies not only diminish the cough, and assist

expectoration, but seem likewise to regulate the secretions throughout the system." 215.

Bronchitis, of every variety, may terminate in a collapse of the powers of the system. If this happen, the strength of the patient must be supported, and the bronchia relieved of the secretions with which they are clogged. Dr. Hastings recommends the employment of ammonia, "whose stimulating powers are least to be dreaded," as the best calculated to produce these effects. "But if a certain degree of collapse have come on, all attempts of the physician to relieve the patient are unavailing."

Topical bleeding and blisters are of importance in acute bronchitis. Whenever, in these affections, general blood-letting is required to be repeated, local evacuation should also be employed. In the second variety, although the symptoms seem to require the employment of venesection, we have often reason to dread its effects; "in such cases, local bleeding should be assiduously used." Blisters should not be applied till the excitement has been reduced by bleeding. When, however, bronchitis occurs in phlegmatic habits, "and assumes the form of peripneumonia notha, blistering may be employed from the commencement, and is one of the remedies to be chiefly relied on in the cure of this variety. In violent and obstinate cases, the blister should be large enough to cover the whole of the anterior part of the chest."

The tepid bath is often of great service in this disease. Local fomentations are also serviceable; "they bring a larger quantity of blood into the vessels of the integuments covering the thorax, and encourage a more copious effusion of blood from the leech-bites."

The author, in the next place, from page 218 to 263, details the histories of nineteen interesting cases of bronchitis, by which the principles and practice of which we have given an analysis, are satisfactorily illustrated.

Chronic Bronchitis. "Chronic inflammation of the mucous membrane of the bronchia," observes our author, "is so often connected with other affections, and so frequently gives rise to extensive mischief in the thoracic viscera, that the consideration

of this disease will necessarily lead us to discuss the nature of some other affections which are occasionally combined with it."

Symptoms. The author divides chronic bronchitis into six varieties. The first variety is, for the most part, a consequence of the first variety of acute bronchitis. The cough generally begins in the commencement of cold weather, and sometimes continues through the winter. "The slightest atmospheric vicissitude produces uneasiness, in consequence of the irritable state of the mucous membrane. The breathing is constantly uneasy, attended with a peculiar wheezing." The cough is most violent in the morning, the patient never failing to cough for a considerable time after he awakes. The cough is apt to be increased by exercise, or after a heavy meal. The expectoration is abundant, consisting of tough mucus, mixed with a puruloid matter. There is seldom any pain felt in the chest. The digestive organs are generally disordered. Pain, and a sense of weight, are frequently felt in the epigastrium. Tongue white and loaded, and want of appetite. Pulse quicker than natural, but seldom hard. The bowels are irregular, and the urine high-coloured, and often scanty. Chronic bronchitis exists sometimes for a long time, without much constitutional derangement.

"The patient is affected with cough, copious expectoration, and uneasy respiration; but there is no fever, and the pulse is not at all accelerated."

"These symptoms occasionally become the foundation of hydrothorax; but more commonly, as the warm weather comes on, the cough subsides, and the patient's health is restored." 266.

2. The second variety resembles tubercular consumption. "The cough is severe, particularly on first lying down, and early in the morning." The expectoration is copious, differing from that in catarrh. Part of the matter expectorated is in lumps, from the size of a pea to that of a common bean. These lumps do not swim when thrown into water; they are viscid, and of a translucent or opaque appearance. Occasionally also, a flaky substance, having a ramified appearance, is expectorated. This kind of matter rarely sinks to the bottom when put in water. A third kind of matter is generally expectorated of a white, yellow, or

greenish colour. It sinks in water, and differs from the other two kinds in the ease with which it is diffused in water by agitation. The matters thrown up are often mixed with small quantities of blood. The breathing is hurried and laborious. A sense of tightness across the breast is frequently complained of. No pain is felt on drawing a deep inspiration. Before rising in the morning the pulse seldom exceeds 90. But when the patient assumes the erect posture, the pulse is commonly immediately raised, becoming as high as 100 or 110. In the evening it is often as high as 120. The heat of the body is constantly above the natural standard. The thirst is greater than in health. The skin is dry and occasionally rough. "The urine is constantly high coloured, and deposits a copious sediment." The face is pale, except during the evening febrile exacerbation. The patient gradually emaciates and loses strength. If the progress of the disease be not checked, debility and emaciation proceed rapidly. The cough augments, and the expectoration becomes profuse. The matter is sometimes expectorated in astonishing quantity; "a pint and a half," says Dr. H., "is sometimes thrown up during the night!" This expectoration consists principally of a yellow or greenish coloured matter, which generally sinks in water, and is diffusible in it, by agitation. There is rarely any pure blood expectorated, though the quantity of blood mixed with the sputa is increased. The dyspnœa is much increased, attended with a sense of tightness and weight across the breast, "and the patient is rendered breathless by the slightest exertion." The frequency of the pulse is increased, being seldom below 120, and in the evening it is much higher. "The tongue becomes cleaner, and in many cases all fur is removed; it assumes a shining appearance, and is redder than in health." The symptoms of hectic fever are now distinctly developed. The evening exacerbations, the night sweats, the diarrhœa, at length exhaust the patient, and he "dies with all the appearances of tubercular phthisis."

We shall pass over the 3d, 4th, and 5th varieties of chronic bronchitis described by the author, as they do not, in our opinion, exhibit sufficiently distinctive features, to enable the practitioner to discriminate them from the preceding varieties. Dr. Hastings

has been justly accused,* we think, of having "refined with unnecessary minuteness," in his subdivisions of both the acute and chronic form of bronchitis. It is impossible at the bed side to make these minute distinctions; and they tend rather to confound than enlighten the judgment of the practitioner. We shall now proceed to notice the 6th and last variety of chronic bronchitis, which consists of such cases as are connected with diseases of the abdominal viscera.

Chronic disease of the liver is one of the most common attendants of this affection of the lungs.

"It very often happens, that in the commencement of this modification of the disorder, we have no symptoms which can lead us to suspect any pulmonic affection. They are altogether such as are usually produced by hepatic disease. We have pain in the right hypochondrium, inability to lie on the left side, irregularity of the bowels, loaded tongue, and depression of spirits." 276.

The first signs of pulmonic disease are very slight. There is cough, which is dry, and without pain. Gradually the cough becomes troublesome, and a tough mucus is expectorated. Respiration is slightly affected, accompanied with a strong sense of weight and stricture across the chest. In the progress of the disease the cough becomes more violent, and the expectoration more abundant. The matter expectorated is still principally mucus. "The expectoration, however, in a more advanced stage, increases in an astonishing manner, and at length becomes purulent." The patient always has considerable tenderness in the epigastric region, attended generally with a dull kind of pain which is referred to the pit of the stomach. An irregular hectic is finally developed, "differing considerably from the completely formed hectic of tubercular phthisis."

"Along with these symptoms denoting affection of the lungs, the disease of the digestive organs is well marked. Flatulence, irregularity of the bowels, furred tongue, impaired appetite, sense of fulness in the epigastric region, almost constantly at-

* *Medico-Chirurgical Journal*, December 1820, p. 366.

tend. The dyspnœa and cough are always worse when the digestive organs are much oppressed. Whenever also the epigastrium is more than usually tender, there is commonly a corresponding increase of the cough." 278.

If the disease be not checked in its progress, "the symptoms approach still nearer to those of tubercular phthisis." Hectic fever is fully developed, and the patient is wasted by the nocturnal sweats. Dropsical symptoms frequently come on, and the patient dies under all the symptoms which are supposed to indicate tubercular consumption.

Our limits will not permit us to give an analysis of the sections on the *post mortem* appearances, and "Ratio Symptomatum of Chronic Bronchitis." Nor can we stop to give an account of the sections on the *diagnosis* and *prognosis* of these affections. We however earnestly recommend an attentive perusal of this part of the work, as it abounds in a great variety of solid observations and judicious precepts.

On the treatment of Chronic Bronchitis. Blood-letting is one of the most powerful remedies for the cure of pulmonary inflammation. In chronic affections of this kind, however, it cannot be employed in all cases with success, in consequence of the general debility which always exists under such circumstances.

"If the disease present itself under the form of chronic cough, in persons not much advanced in life, whose constitution is strong, and the pulse is full, inclined to hardness and frequent, we may always detract a small quantity of blood. But large blood-lettings are improper." 298.

In such cases local is perhaps better than general blood-letting. When in young subjects catarrh has existed a long time, and symptoms resembling the first stage of tubercular phthisis have come on, bleeding is improper. Where the disease has been kept up for many years by the peculiar occupation of the patient, one copious venesection is sometimes of great benefit.

"Under such circumstances it occasionally happens, that the constant obstruction to the free circulation causes enlargement of the right side of the heart, and a general plethora of the ve-

nous system. The patient's breathing becomes laborious, the face and hands purple, and every symptom indicates an obstructed circulation through the lungs. One large venesection sometimes removes all the urgent symptoms; and by discontinuing the employment which has given rise to the affection, and by the application of the remedies to be afterwards mentioned, such patients are generally restored to a comparatively good state of health."

Vesicatories and rubefacients applied to the chest are often of considerable service after the force of the circulation has been reduced. They cannot, however, be safely used where the inflammatory symptoms are strongly marked. When the pulse is not very hard, and the patient is not very excitable, blisters may be applied immediately after the leeches' bites have ceased to bleed. By this practice we often succeed in relieving the cough, and the purulent secretion from the bronchial membrane is sometimes converted into the natural mucus of the part. If the disease is obstinate, we often derive more benefit from a *seton* or the application of caustic to the chest, than from blisters.

Whenever the lungs appear loaded with mucus, as in that variety of chronic bronchitis which is denominated *tussis senilis*, emetics are generally of considerable temporary advantage. They do not seem to forward a radical cure. When the disease in question assumes the form of catarrhal phthisis, digitalis often affords much relief. This medicine is not, however, applicable to all cases of chronic bronchial inflammation. "It is highly beneficial in all those examples in which bronchial inflammation has a tendency to terminate in dropsy; but when the disease is attended with much prostration of strength, digitalis, even if indicated, must be exhibited with great caution."

When the disease has assumed the character of chronic cough, squills are often very serviceable. The author has been in the habit of combining this remedy with ammoniacum, as directed in the compound squill pill of the London Pharmacopœia; "and in old people of phlegmatic habits, when there is not much fever, he has found this a very useful expectorant."

“ The tincture of meadow saffron appears a very promising remedy in chronic bronchitis, and certainly possesses remarkable powers. It allays the cough, promotes the flow of urine, and keeps up a regular alvine discharge. It can be given much more generally than squills, because it does not produce that feverishness which results from the use of the latter remedy, and can therefore be employed where there is considerable fever; for, from the power it possesses over the secretions, this medicine tends to relieve fever. The dose generally prescribed by the author is twenty drops three times a day. In some cases this must be diminished, on account of its action on the bowels, severe diarrhœa being occasionally brought on by it. Sometimes the patient is not affected by twenty drops; if this should happen, the dose may be gradually increased, until the bowels, the skin, or the kidneys, are acted upon.” 304.

Dr. Armstrong thinks that the balsam copaiva deserves to be conspicuously placed among the internal remedies for chronic inflammation of the bronchial membrane. Dr. H. does not ascribe to it such virtues in any remarkable degree. He states that he derived very beneficial results in chronic bronchitis from the combination of cicuta with ipecacuanha powder. “ A pill containing four grains of extract cicuta and one of powdered ipecac. taken three times a day, often allays the cough, and produces a more healthy expectoration.”

In certain states of the system cinchona produces good effects. It is particularly applicable to those instances that succeed acute bronchitis, “ where the weakness brought on by the acute attack is very considerable.”

If chronic bronchitis be combined with hepatic affection, very small doses of calomel should be exhibited with other remedies, and continued until the gums become slightly red, or till the tenderness of the epigastrium is diminished, and the stools become more natural. Mercury should always be given in such cases, so as to produce the least possible debility of the system. Dr. Philip, in his observations “ on a species of pulmonary consumption,” strongly recommends the mercury to be given in combination with dandelion. He does this chiefly for the purpose of lessening

the quantity of mercury necessary to be given in such cases. In those cases that sometimes occur after measles, "when the shrillness of the voice indicates considerable affection of the mucous membrane lining the trachea," calomel may be used with much advantage.

Opium is only a palliative, and inadmissible so long as there is much fever. In the tussis senilis, it is, however, often indispensable. A dose may be given at bed time; but we must be cautious in its use, lest, by preventing expectoration, it prove prejudicial. Dr. Duncan recommends lactucarium, in cases where opium cannot be given.

"Of all the medicines," says he, "which I have employed for alleviating cough in phthisis, and indeed as a sedative in many other diseases, next to opium, I have found no article so beneficial as that substance which some have lately denominated lettuce opium, and which I term lactucarium."*

The inhalation of the vapour of tar, has done some service in the author's practice in *chronic inflammation of the bronchia*; but "not one instance has occurred to him, of even temporary relief of this application, in tubercular consumption."

"From the author's experience of this remedy, it appears, that when the habit of body is irritable, and the inflammation at all active, the symptoms are increased by its use; but if the disease have been long in a chronic state, and the habit of the body be not irritable, relief follows its application." 309.

The author, in conclusion of this part of his work, gives some directions relative to regimen. "As much attention," says Dr. Hastings, "is requisite to the air which the patient breathes, as to the aliment he feeds upon. There is no disease to which the maxim of Celsus '*Pessimum ægro cælum est, quod ægrum est*,' is more applicable than the present." A change of air has often cured obstinate cases of bronchitis which had resisted all medical aid.

From page 312 to 367, twenty-one cases of chronic bronchitis

* Observations on Pulmonary Consumption. By A. Duncan, Sen. M. D. 2d edit. p. 162.

are detailed, illustrating in a very interesting manner, the principles and practice which are previously given. In the concluding chapter, the author treats of "*Dropsy, as dependant on inflammation of the mucous membrane lining the bronchia.*" But we have already drawn out this analysis to so great a length, that we are obliged, from want of room, to omit giving an account of the remaining portion of this valuable work. There is much condensation and clearness in the style of this work, and it bears internal evidence of the author's great talent for observation, closeness of research, and correct judgment.

E.

ART. XIII. *Practical Observations on the Use of the Cubebs, or Java Pepper, in the cure of Gonorrhœa, with Cases.* By HENRY JEFFREYS, ESQ. Senior Surgeon of the St George's and St. James' General Dispensary; Assistant Surgeon to the Lock Hospital, &c. 8vo. pp. 67. London, 1821.

FROM the earliest days of our science, new remedies have risen like bubbles, in constant succession, which, after having attracted, for a time, the attention of the profession, and excited its most extravagant expectations, have again sunk into insignificance and neglect. In a science like ours, depending mainly, if not wholly, upon experience for its solid improvements, this must of necessity continue to be the case as long as we make any efforts to extend the sphere of our knowledge, concerning the value and peculiar powers of the remedial agencies, which nature has so abundantly spread over the face of the earth. If, in the long search after the means to remove or alleviate disease, we shall find but one article, which is better calculated to this end, than those we already possess, we need not care for the many disappointments, which lie in our way, in searching for it. We cannot, therefore, chime in with those who are habitually vociferating against the introduction of new remedies, under the plea that they are generally, in the end, found useless. It is the duty of every practitioner to entertain a due share of scepticism in

relation to all new remedies; but we believe it to be equally his duty to encourage the inquiries which are directed to the discovery of their real virtues; and to treat with circumspection, but not with neglect, the experience of others, when offered in testimony of the remedial virtues of a new article.

With regard to the remedy which is the subject of the work under review, we must confess, that our own experience does not speak much in its favour. It has, however, in a few instances, been of decided advantage in our hands in the treatment of gonorrhœa; though in the majority of cases it did no good.

The author of this work "has had extensive opportunities of putting the powers of this new remedy to the test;" and the testimony which he offers in its favour, is therefore to be regarded as of considerable weight.

"What may have been the success," says the author, "it has met with in the hands of others, I am not prepared to say; but in my own practice it has been such as entirely to remove any diffidence with which I first made trial of it; and to induce me to consider it, not only as a very safe remedy, but, in the generality of cases, infinitely more useful and expeditious than any which has ever yet been introduced into practice for the cure of gonorrhœa."

It appears that in India the cubebs have long been employed in the cure of gonorrhœa. They were first noticed in England as a remedy for this disease, in a communication by Mr. Crawford, published in the Edinburgh Medical and Surgical Journal for January, 1818. Another paper appeared on this subject, by Mr. Adams, in the same journal for January, 1819.

The plant which affords this article (*Piper Cubeba*) is indigenous to Java. The berries are the only parts of the plant employed in medicine. When dried, they are of a light brown colour, wrinkled on the surface, about the size of common pepper, and provided each with a slender stalk or pedicle; and hence they are sometimes called *piper caudatum*.

This article contains a very large portion of an essential oil,

in which its medicinal properties seem entirely to reside.* It should always, therefore, be kept in stopper bottles, when in a pulverized state. "Proof spirit digested on the berries, takes up nearly the whole of the oil, and the tincture which is thus formed, has a clear reddish brown colour, and possesses all the virtues of the cubebs." The author gives the following formula for the *TINCTURA CUBEBAE*.

R Bacc. Piper. Cubeb. \mathfrak{z} iij.
Spiritus Vini Tenuoris, O j.

Digere per dies septem, et cola.

When taken into the stomach this article does not produce any very remarkable action on the system.

"Like other spices, it is warm and stimulating: in some constitutions it proves mildly aperient, probably in giving tone and vigour to the bowels; in others it has a contrary effect, and castor oil, or some other gentle laxative, is required to be taken occasionally during its exhibition. In some persons, to whom I have administered the Cubebs, it has produced a considerable degree of headach and nausea; and in one instance (Case XX) the patient was so annoyed by these symptoms as positively to refuse going on with the remedy. When given in large doses, as a drachm and a half or two drachms of the powder four times a day, it appears to increase the secretion of urine, which assumes a deeper tinge of colour than usual, is voided somewhat more frequently and in larger quantity at a time, and acquires a peculiar and slightly aromatic odour, which is not unpleasant."

The author thinks that its "exhibition will rarely or perhaps never be followed by those consequences, which so often take place under the ordinary mode of treatment, when the discharge is suddenly checked." He is also of opinion, that it is in the

* "The following analysis of the seeds of this plant, by M. Vauquelin, is given in the London Medical Repository for December, 1820, p. 523. "They contain, 1. A volatile oil, almost concrete. 2. A resin, resembling that of the balsam copaiva. 3. Another resin, but in small quantity, and coloured. 4. Extractive principle, similar to that found in leguminous plants. 5. Saline substances."

more inflammatory forms of the disease in question, that its sanative powers are more certainly displayed.

As the good effects of this article, according to the author's experience, commonly begin to show themselves "within forty-eight hours after the exhibition of the first dose;" he thinks that if it does not afford some material relief in the course of five or six days, it will rarely be attended with advantage.

"The period," says the author, "in which it has effected a cure, in those cases wherein it has succeeded under my management, has varied from two or three days to a fortnight or more. In many instances, it has proved to be of considerable efficacy where other remedies had been tried in vain, and where the disease was of some standing. Out of the first twenty-one cases in which I prescribed it, taken indiscriminately as they occurred, fourteen were cured, four relieved, and in three it failed."

The author asserts, that in those cases where this remedy afforded only partial relief, it appeared to him to bring on such a state in the parts affected, as to cause the symptoms of the disease to yield more readily to copaiva, than under ordinary circumstances. "When the more urgent symptoms (says Mr. J.) have been allayed, it has appeared to me that the cure has been accelerated by the use of a mildly astringent injection."

We will now subjoin a few cases from the author's book, illustrative of his mode of employing this article, and of its decided medicinal powers in gonorrhœa.

Case IV.—A married man, aged about 50 years, had gonorrhœa, ardor urinæ, and chordée, with a profuse discharge, and great pain and uneasiness in the perinæum. He had been ill eight days, and had made use of remedies without any relief.

He took a dessert spoonful of the cubebs three times a day; and in four days all his painful symptoms, together with the discharge, had entirely subsided.

The medicine kept his bowels rather more open than usual.

Case IX.—An officer of hussars, whose regiment was quartered in Ireland, had been nearly three months ill with gonorrhœa, for which he had used a variety of remedies to no pur-

pose, when he was obliged to come to London on business, and travelled four or five nights together in the mail. In passing through Dublin he had connexion with a prostitute; and on his arrival in town, he found that his complaint was as bad as ever. He went into a warm bath the morning of his arrival, and in the afternoon called upon me. He had a profuse discharge, pain and scalding in making water, and a good deal of chordée; and general uneasiness about the penis, perinæum, and groins.

He took a drachm of Cubebs four times a day, and went about transacting the business which had brought him to town.

Four days afterwards he called on me again, equally pleased and astonished at his rapid amendment: the discharge was scarcely perceptible, and he had no other symptom left.

Case XV.—John Ellis, aged twenty-seven years, was admitted on the books of the Dispensary, Sept. 21, 1819, with gonorrhœa, ardor urinæ, chordée, and a painful enlargement of a gland in the right groin, as big as a filbert. He said he had been ill a week.

He applied a cold, saturnine lotion to the penis, and took a drachm and a half of the tincture of cubebs four times a day.

Sept. 25. The symptoms were very little relieved, and a drachm of the powdered cubebs was substituted for the tincture.

Sept. 28. The discharge and ardor urinæ had nearly subsided, but he still complained of chordée at night: he was, therefore, ordered to take a drachm of the vinum colchici at bedtime, for two nights; but by mistake he took it in the morning. This medicine purged him, and excited nausea; but he had less chordée at night; and on the 30th scarcely any discharge could be observed.

Oct. 5. The discharge had returned in a slight degree. Co-paiva was now substituted for the cubebs; and in four days no appearance of complaint was remaining.

Case XVI.—Francis Simkins, aged thirty-four, had a purulent discharge from the urethra, without any other symptom, of about a week's duration. He took the tincture of cubebs, and at the end of a week was quite cured.

This man complained of headach and giddiness while taking the medicine.

The following table exhibits the result of twenty-seven cases of gonorrhœa treated with the cubebs.

| | | | | | |
|-------------------------------|---|---|---|---|----|
| Total number of cases treated | - | - | - | - | 27 |
| Of which were cured | - | - | - | - | 18 |
| relieved | - | - | - | - | 6 |
| Failures | - | - | - | - | 3 |
| | | | | | 27 |

Of those which were cured,

| | |
|------------------------------|------------------------------|
| Case I. was cured in 2 days. | Case X. cured in a few days. |
| II. 9 | XI. 8 |
| III. 12 | XII. 6 |
| IV. 4 | XIII. a short time. |
| V. 14 | XVI. 7 days. |
| VI. 9 | XXII. 7 |
| VII. 7 | XXIV. 18 |
| VIII. 3 | XXVI. 17 |
| IX. 4 | XXVII. 6 |

The cases relieved were,

XV.
XVII.
XVIII.
XIX.
XXIII.
XXV.

The failures were, cases

XIV.
XX.
XXI.

Since the above review was written, we have given the cubebs in two cases of gonorrhœa. One was a recent case; the other had been of long standing, and for which the usual remedies had been assiduously, but ineffectually employed. The recent case yielded to this remedy in 8 days; the other case was cured by it in about two weeks. The tincture was prescribed in the dose of a large tea-spoonful every 4 hours, during the day.

E.

ANALECTA.

Periodical suppression of Urine.—The following singular case of suppression of Urine, caused by Tea, Tobacco and Gin, is related by Mr. Harrison Wilkinson, in the 86th number of the London Medical Repository.

About two years ago suppression regularly occurred every night, about twelve o'clock, accompanied by very excruciating pain. The bougie always afforded the desired relief; but it was sometimes necessary to use it a second time. Various diuretics and antispasmodics were exhibited without producing any beneficial effects. As experience decided that medical and surgical agency were inadequate, so I most scrupulously investigated the probable effect of diet. From various reasons, superfluous to mention, I formed the opinion that drinking tea might be a cause of the retention. As my patient uniformly experienced the soothing and exhilarating effects of that beverage, he consented to its omission for one single night with great reluctance. He escaped his ordinary midnight attack, yet was still unconvinced of the deleterious effect of tea upon his urinary organs. In the following evening he drank tea as usual, and at midnight had his ordinary paroxysm. Next night he was prevailed upon to omit his tea, and a second time he escaped. Such striking experiments could not fail to remove his scepticism; and he remained entirely free for near six months. The fondest attachment to a beverage, most deleterious in its consequences in his case, seduced him from his resolution, and about five hours afterwards a suppression of urine ensued; and for the removal of which, he was compelled, after suffering two hours' torture, to introduce a bougie, and a second during the same night. I laid it down to him as an invariable rule never to use a bougie until the last extremity. From that time he was free from any attack for about five months, when the attack returned nearly every night. For nearly three months he was compelled to use every night a bougie about twelve o'clock, and sometimes even a second. Anodynes and antispasmodics were of no service. Still complained much of dyspepsia, and occasionally of wandering rheumatic pains. Diuretics, except the tincture of colchicum, were, as usual, unavailing; while the latter, which was only for two or three nights, and is allowed to be an exception to the above, acted upon the bowels as a hypercathartic. He succeeded, without a bougie, in emptying the bladder.

At that time he every evening had broma or cocoa instead of tea, and afterwards drank gin and water and smoked tobacco. The failure of the ordinary plan of treatment induced me to consider if no other source could be discovered that acted as an exciting cause. Embarrassed by nightly paroxysms, and disappointed in former expectations, I was induced to recommend that the gin and tobacco should for one night be left off. I overcame the prejudice of my patient with some difficulty. He was of opinion that my plan could not relieve him; but, out of deference to my opinion, he consented to accede to my proposition, and the first night he escaped his regular attack. Doubts about the efficiency of my plan, and predilection for gin and beer, induced him to return to his previous habit; and he was again attacked, sometimes in an hour after the gin and tobacco, but more commonly about three. The third night, he had neither gin nor tobacco until after the ordinary time of attack, and the paroxysms came on three hours later, and a bougie was used. This produced conviction upon his mind, and a resolution has been entered into not to return again to the exciting cause. He is a person of sober habits, and was induced to take gin and tobacco, under an idea that his digestive organs were relieved. He never drank any gin or used any tobacco during the day. He has been entirely free from his attack for a long time.

On the Mechanism of Absorption in red and warm blooded Animals.—Dr. Magendie says, that the results of experiments related in his former memoirs on this subject show that—

“ 1°. The sanguiferous veins are endowed with the absorbent faculty.

“ 2°. It is not demonstrated that the vessels which absorb the chyle can absorb other matters.

“ 3°. The absorbent power of such of the lymphatic vessels as are not chyloferous, is not yet established on sufficiently satisfactory evidence.”

Then follow remarks, that these inferences are qualified to explain how it is that substances are absorbed in parts devoid of lymphatic vessels, (as Dr Magendie thinks proper to say,) as “ the brain, the eye, &c. ;” and an attempt to ridicule the notions that the supposed absorbent vessels exert a sort of “ discernment” in the selection of the fluids they transmit, (which ridicule is just as well applied here, as it would be to the notion that the larynx has a sort of “ discernment,” since it admits air and refuses to admit brandy; or that the stomach possesses such a faculty, since it retains and digests a potato, and rejects a root of ipecacuanha.)

Dr. Magendie then states that his experiments have shown, that when a certain quantity of warm water has been injected into the veins of an animal, absorption is impeded; and when the quantity of blood is diminished by abstraction, absorption is accelerated; and that, when a certain quantity of blood is abstracted, and replaced by the same quantity of warm water, absorption is effected as rapidly as it is under ordinary circumstances. These facts have led the author to infer, that absorption is a purely physical action in the animal economy, and depends on “ the capillary attraction of the vascular parietes for the matters absorbed.” After having adduced some considerations which he thinks favour this explanation, he proceeds to relate the results of experiments which he believes prove it to be true. This explanation, he remarks, would indicate that absorption should be effected by vessels after death, as well as during life; and this he considers as also proved by his experiments. Dr. Magendie says—

“ I took a portion of the external jugular vein of a dog: (this portion of vessel, in length nearly an inch, received no collateral branch:) I deprived it of surrounding cellular tissue; I attached to each of its ends a glass tube, by means of which I established a current of warm water in its interior. I then plunged the vein in a slightly-acid liquor, and I carefully collected the fluid which formed the interior current.

“ It is evident from the disposition of the apparatus, that there could be no communication between the interior current of warm water and the exterior acid liquor.

“ For the first few minutes, the liquor which I collected had suffered no change; but, after five or six minutes, the water became sensibly acid. Absorption had then taken place.

“ I repeated this experiment on veins taken from dead human bodies; the effect was the same.”

Similar results were obtained from similar experiments with a portion of an artery, when it was besides remarked, that “ the more acid the external fluid, and the more high the temperature, (below a certain distance from the boiling point,) the more rapidly the phenomenon was produced.”—“ If capillary absorption occurs in large dead blood-vessels, why should it not take place in similar living vessels?” Aware of the obvious reply of a sceptic, Dr. Magendie put this question to the test of experiment. He says—

“ I took a dog about six weeks old, at which age the vascular parietes are thin, and consequently particularly calculated for the success of the experiment. I laid bare one of the jugular veins, isolated it completely throughout its whole length; carefully separated it from the parts adjacent,

especially the cellular tissue, and some small vessels which ramified from it; and placed a card beneath it, in order that the vessel might have no contact with the surrounding part. I then let fall on its surface, at the middle of the card, a thick aqueous solution of an alcoholic extract of *nux vomica*, a substance the action of which on dogs is very energetic: I took care that no portion of the poison should touch any other part than the vein and the card, and that the course of the blood was free in the interior of the vessel. Before the lapse of the fourth minute, the effects which I expected were manifested, at first but feebly, but afterwards with such severity, that insufflation of the lungs was necessary for the preservation of the life of the animal."

Similar results were obtained, on repeating the experiment on an adult dog. It remained to try it on an artery. This was effected on the carotid arteries of two large rabbits. The effects were developed in both animals, and one of them died. "To assure myself," Dr. Magendie says, "that the poison had really traversed the parietes of the artery, and that it had not been absorbed by the small veins which might have evaded my dissection, I carefully detached the vessel which had been the subject of the experiment; I divided it throughout its length; and I persuaded the persons who assisted me to taste the little blood which had remained adherent to its interior surface: they all recognized, as well as myself, the extreme bitterness of the extract of *nux vomica*."

In order to ascertain whether the minute vessels also permitted fluids to traverse their parietes, Dr. Magendie made the following experiment:—"I took," he says, "the heart of a dog who had died on the preceding evening; I injected into one of its coronary arteries, water of 30° centig. (85° Fahren.) This water readily returned by the coronary vein into the right auricle, whence it flowed into a vessel. I poured into the pericardium half an ounce of water slightly acidulated. At first the injected water showed no sign of acidity; but, on the lapse of five or six minutes, it presented unequivocal evidence of the possession of that quality." Thus, the fact in question appeared to be proved in respect to dead minute vessels. In regard to living vessels of this class, Dr. Magendie says, "It was not necessary to have recourse to new inquiries, nor to sacrifice more animals. The experiments which I mentioned in my memoir on *the Organs of Absorption in the Mammalia*, left no doubt on this point, according to the judgment of the Academy."

In respect to the permeability of membranes by certain fluids during life, (evidence of which after death we have in the colouring of the parts adjacent to the gall-bladder with bile,) Dr. Magendie observes, that he has often seen membranes penetrated and coloured by substances placed in contact with them: for example, if a certain quantity of ink is introduced in the cavity of the pleura of a young dog, the period of hardly an hour is requisite in order that the pleura, the pericardium, the intercostal muscles, and the surface of the heart itself, may be rendered black.

Amongst his inferences from the foregoing facts, Dr. Magendie remarks, that "the capillary attraction of the parietes of the small vessels appears to be the cause, or, more exactly, one of the causes, of the absorption called venous;" whilst he takes care to add, that "this conclusion does not 'touch' in any respect the absorption which is effected in the small intestine on the chyle, by the chyliferous vessels, and still less the absorbent property of the lymphatics; though the experiments I have just described seem to indicate, that if, in the greater number of instances, these vessels do not absorb, this depends not on their parietes, which have properties similar to those of the veins, but to the want of a continuous current in their interior."

Piper Cubeba in Leucorrhœa.—Dr. Traill of Liverpool makes the following remarks, in relation to the utility of this article in leucorrhœa.

Having proved the utility of the Indian remedy, *piper cubeba*, in many cases of blenorhœa, I was led by analogy to try it on several persons labouring under leucorrhœa, and am satisfied that it is a valuable auxiliary in this troublesome disease. In every instance it has mitigated the violence of the complaint, and in several cases has removed it. I have administered it in doses from 1 to 3 ℥ of powder; but many persons cannot take more than ʒi. without experiencing nausea; and ʒij. given twice or thrice a-day, will generally be found sufficient. As far as my experience goes, it is most useful in old and obstinate cases of blenorhœa: in one recent case it seemed to have a tendency to excite hernia humoralis.

Remarks on Coffee as an expectorant, by James R. Scott, F. R. S. Edinburgh.

—As that state of the bronchial membrane, in which there takes place a pretty copious and free secretion of mucus, is unfavourable to the continuance of certain paroxysms of asthma, it becomes desirable to produce such a state, the symptom or indication of which will be as above, and therefore various medicines called expectorants are used. Coffee as a palliative in asthma has been long known. Some think it chiefly acts as an antispasmodic; but I was about a year ago forcibly struck with its singularly sudden operation as an expectorant in my own person; I found almost every expectorant fail in its effect but coffee. A friend observed and called my attention to the fact, that the moment I swallowed about half a tea-cupful of coffee without milk, I expectorated freely. I have since that time derived great advantage from its use; but I always find that the expectoration instantly succeeds to the introduction of it into the stomach, however dry or even sore the bronchial membrane may be. No other medicine of the class of expectorants operates at all so speedily on me as coffee. How closely then must nervous sympathy be connected, and how quickly the nerves act over secretion?

Experiments on Hydrophobia, by M. Magendie, M. D.—M. Magendie remarks that, both in animals and men labouring under Hydrophobia, the most active substances, the most powerful narcotics, have no perceptible operation. This holds good, not only as to matters taken into the stomach, but injected into the veins. For instance, he has injected into the veins of dogs that were hydrophobic large doses of opium, (ten grains) without any perceptible narcotic effect; while a single grain produced eight or ten hours of somnolency in a healthy animal of the same species. It was the same in man. M. Dupuytren and our author injected into the radial vein of a young man, in rabies canina, about eight grains of the gummy extract of opium, without any apparent result. Prussic acid was also injected into the vessels of dogs, with the same want of effect. M. M. Magendie and Breschet inoculated a healthy dog with the saliva of the young man above-mentioned, by inserting some of the fluid under the skin of the forehead. The animal became mad at the end of a month. Two dogs, bitten by the latter, became affected with hydrophobia in forty days. These last bit several other dogs, but without effect. So that, according to these experiments, the virus becomes innocuous in the third inoculation or generation. But to come to the main experiment of this paper.

The proprietor of a kind of menagerie, in Paris (*le combat des animaux*), sent for M. Magendie to see a very large and strong bitch, in a high state of rabies. The constant agitation of the animal—hoarse and short barkings—and fierce expression of the eye, convinced our author that the animal was hydrophobic. Early next morning, M. Magendie, attended by several of his most zealous pupils, secured the animal, with some difficulty and hazard.

M. Magendie then opened the left jugular vein, and drew off about sixteen ounces of blood; after which, he injected nearly forty ounces of water; during the latter part of the operation, however, permitting ten or twelve ounces of blood and water to flow from the upper part of the orifice. The injection finished, the dog was let loose into her den; and to their great surprise, coiled herself up and lay down, as if to sleep, in the most perfect state of calmness. The fierce expression of the eye was entirely gone—she did not bark, and only ground the teeth, when a stick was put into her cage. M. Magendie waited an hour, during which the animal lay perfectly quiet. Some pupils were left to watch her. About five hours afterwards, she was seized with a difficulty of breathing, which increased, and killed her in half an hour more. On dissection, the brain, spinal marrow, and all the organs, excepting the lungs, were sound. The lungs were gorged with watery blood, and the mucuous membrane appeared inflamed. M. Magendie, before the fatal termination of this case, accused himself of having injected too much water, and anticipated effusion in the lungs. What led him to this experiment? It was from observing that, in artificial aqueous plethora, the various functions of the animal, especially those of the nervous system, were very evidently enfeebled. Now, in rabies, the excitement of the nervous system is carried to its utmost limit; and hence he was naturally enough led to try the sedative effects of aqueous injection and bleeding. Moreover, from the time an animal becomes mad, he ceases to drink; while the pulmonary and cutaneous transpirations are in full force. Hence he found the blood of rabid animals thick, and apparently without serum. Upon the whole, this experiment, though unsuccessful, holds out a ray of hope in this hitherto incurable affliction.—*Magendie's Journal of Physiology, No. I, January, 1821.*

Curious Phenomena resulting from blood-letting in a Horse.—Mr. Bouley, a very able veterinary surgeon of Paris, bled a horse, having pneumonia, in the neck, with the phleme, in the usual way. Nothing particular occurred during the early part of the operation: but, as the vessel into which the blood was received was not large enough to contain the quantity which Mr. Bouley wished to take, he, on this vessel being full, suspended the compression on the vein below the puncture, whilst the vessel was emptied. At the instant when the compression ceased, he heard a remarkable noise, which he had several times noticed in the course of his practice, without any ill consequence following the event, and to which he now, therefore, paid but little attention. The bleeding was completed, and the animal led into his stable. He had but just arrived there when he was affected with a general trembling; his breathing became laborious and *plaintive*; his pulse small, irregular, and much accelerated; and, finally, he uttered some deep groans, and fell down in his stall “as if stricken by lightning.” On reflecting on the whole of the circumstances of the case, Mr. Bouley believed that the noise he heard, above alluded to, arose from the rushing of air into the vein; and he instantly determined to draw more blood from the animal. As the blood flowed, the horse “appeared to assume a new life:” he made some efforts to get on his legs, but did not succeed until the lapse of five or six minutes from the last bleeding. When up, his pulse became sensibly developed, lost its rapidity; his breathing became deeper; and, in half an hour from the time of the accident, he seemed to be in “the same state as before the first bleeding.” Some new phenomena were now observed. The horse experienced, during the whole of the afternoon of the same day, “an extreme degree of sensibility of the whole of the right side of the body, (the side opposite to that in which the venesection was practised,) accompanied with very intense pruitus: he laid down and rolled himself about on this side, to rub himself against any objects that offered resistance.”

The pneumonia run its ordinary course, and terminated favourably.—Thirty days after the accident the horse was put to his ordinary work, and has not since shown any sign of disease.

This case is related in the 2d number of Dr. Magendie's Journal, and Dr. M. remarks that Professor Dupuy, of Alfort, has mentioned to him that he had witnessed a similar accident, in which a second bleeding was also immediately effected. This case terminated favourably. Dr. Magendie doubts whether sufficient air was introduced to have proved mortal if the second blood-letting had not been resorted to. He injected some air (he does not say how much,) into a vein of a dog, and then bled him; but the animal died as soon as if he had not been bled immediately after the introduction of the air.

Case of Extirpation of the Thyroid Gland, by Dr. Klein, of Stuttgardt.—A boy, eleven years of age, deaf and dumb, and of a very delicate constitution, had, from his infancy, a circumscribed tumour situate on the left side of the neck, about the size of a nut, and which had gradually increased in bulk. After having been put under the care of several practitioners, he was brought to Dr. Klein. The tumour commenced on the left side, at the margin of the left jaw, occupied the whole of the left side of the neck, as far as from the larynx to behind the ear, and down as low as the third rib. Vessels of the size of the finger run over its surface: it was moveable with difficulty, rather because of its weight than of its adherence to the parts beneath. It was a little irregular, or tuberculated, in its construction; and the pulsation of the arteries which penetrated its interior could be distinctly felt in several parts. Its removal by the knife having been determined on, the patient was laid on a table, so that his head, shoulders, and legs, might be most effectually secured by assistants. On making the necessary preliminary incisions in the integuments, the veins, which were much dilated, were divided, and threw out about half-a-pound of blood; but they were soon secured by compression with the fingers, as well as the subclavian artery. After the flaps of the integuments were dissected aside, the tumour was forcibly drawn outwards; and, using sometimes the fingers and sometimes the scalpel, or its handle, it was separated from the larynx and trachea. The operation occupied a minute and a half; but Dr. Klein was surprised to find no hæmorrhage, even from the thyroid arteries, which were, of course, divided. On examining the patient, however, he was found to be insensible; and, though every proper means was used for three-quarters of an hour, it was found impossible to restore him to life. On opening the body, it was found that neither the carotid artery, the par vagum, the jugular vein, nor the trachea, had been injured; but all the vessels of the dura mater and the brain were gorged with blood: there was a large quantity of serum in the ventricles. "I think myself authorized," says Dr. Klein, "to conclude that the patient died of apoplexy occasioned by the revolution in the circulation which the operation had caused. The tumour itself was found by the thyroid gland, which had suffered disorganization throughout its whole structure, excepting a small portion on the right side. It weighed two pounds, and its base was of about six inches diameter in every direction. It was formed of a solid, lardaceous, reddish substance, divided into several lobules."

In regard to the cause of death in this case, we would suggest to others the consideration whether or not *moral* circumstances had some influence in causing it. The agitation and suffering of the boy,—being deaf and dumb, and consequently incapable of understanding the object or necessity of the operation,—must have been extremely intense. We doubt whether such an operation on such a subject was justifiable.

Sulphate of Platinum a Test for Gelatine.—Mr. E. Davy recommends the use of the sulphate of platinum in detecting small quantities of gelatine. From comparative experiments made with it, and astringent infusions, he found, that when the quantity of gelatine was so small as not to be affected by strong infusions of oak-bark, nut-galls, or catechu, still there was an immediate precipitate on adding the sulphate of platinum. Where the proportion of gelatine was so reduced as not even to affect sulphate of platinum at first, the precipitate was immediately produced on boiling the fluid.

The different astringent infusions, as of oak-bark, nut-galls, catechu, &c. do not act uniformly on the various kinds of gelatine: thus, an infusion of catechu would produce no precipitate in solutions of paper-hangers' size; but the sulphate of platinum acts equally on all kinds of size, and throws down precipitates which appear to be always similar, not being affected even by the presence of free acid in the solution.—*Journal of Science, &c.*

On Iodine, and its Existence in Sponge.—We inserted, in a late number, a paper by Dr. Coindet, on the use of iodine as a remedy for bronchocele; and gave, at the same time, an account of Mr. Fife's discovery of its existence in sponge: we now find that Mr. Straub, of Hofwyl, as early as December 1819, appears to have shown its existence in this substance, and proposed the use of it instead of the *spongia usta* in medicine. In order to obtain the iodine from sponge, the latter, after being burnt, was washed with water, and the solution decomposed by sulphuric acid; and in this way so much was obtained from half an ounce of sponge as to confirm the ideas previously entertained, that its medicinal properties were owing to this substance.

Mr. Straub recommends trials of preparations of iodine in medicine, and thinks that, where salts formed from it cannot be obtained, an alcoholic extract of burnt sponge is much to be preferred to the burnt sponge itself.

Mr. Straub also asserts the existence of iodine in turf. He was led to examine this substance in consequence of the peculiar odour he observed in the neighbourhood of those buildings where turf is burnt. Repeated experiments confirmed this conjecture; and, by acting on two pounds of turf, abundant evidence of the existence of iodine in it may be obtained. It was found also in the cinders of the *helminthocorton*, though in very small quantities.—*No. 264 of the London Medical and Physical Journal.*

On the internal Use of Acetate of Lead.—Dr. Fouquier, of Paris, to whom we owe the introduction of *nux vomica* into use, has lately been trying the efficacy of another powerful remedy—the *acetate of lead*, when given in larger doses than those in which it had hitherto been administered. "When a physician must renounce all hope of curing his patients," says Dr. Fouquier, "there often yet remains for him this consolation, that of alleviating their sufferings, and prolonging their existence." The profuse sweats in phthisis contribute much to exhaust the patients of that disease, and hasten the fatal termination, especially when accompanied with colliquative diarrhœa. Dr. Fouquier thinks that he has found in the acetate of lead a mean of repressing this debilitating evacuation. He first employed that medicine in the solid form, in pills of a grain each; but he has since found it more convenient to administer it in solution, in a mucilaginous mixture. He relates the histories of twelve cases, arrived at the last stages, in which this medicine was used, in one to the extent of twelve grains as a dose, without finding after death, in the patients who died in the hospital, any affection of the intestines which might be attributed to the mineral. In one or two individuals only were some slight colic pains manifested; and these did not appear to be owing to the medicine, as they were accompanied with diarrhœa, and did not vary whether or not the use of it was continued. In a few cases there was constipation, for the removal of which glysters became necessary.

Tic Douloureux cured by Carbonate of Iron.—In the 89th number of the London Medical Repository a case of Tic Douloureux is related by Dr. W. W. Carter, which, after a variety of the usual remedies had been employed ineffectually, yielded readily to the carbonate of iron given in scruple doses repeated every fourth hour.

Extract of a letter from G. v. D. BUSH, M. D. of Bremen, to Dr. EBERLE, dated Bremen, May 20, 1821.

"I have lately seen several cases of mania à potu, in which I gave opium with the greatest benefit. This article, I believe, should never be used in this disease, unless it be given in large doses. I generally begin with two grains, and repeat it in the dose of one grain every hour, until sleep be induced. My late friend, Dr. Albers, gave emetics according to Dr. Klapp's method. In some cases he derived advantage from this practice; but in many he was obliged to return to the opium. I have dissected the bodies of two persons who died of this disease, but have not found any disorder in the brain. It appeared to me, however, that the brain was firmer than usual. The disease is not rare in our city, and I shall, in the next case that falls under my care try the emetics. I have lately translated Dr. Klapp's very valuable papers on this affection, for the "*Journal for Practical Medicine, conducted by Professor Horn, of Berlin, and Professor Nasse.*"

Extract of a letter from the late Dr. ALBERS, of Bremen, to Dr. EBERLE, dated Bremen, February 17, 1821.

I have lately received several valuable French publications, of which the works of Lallmand and Rastan, "*Du ramollissement du cerveau,*" appear to me exceedingly interesting. The name, however, is very badly chosen; for the disease is a *chronic inflammation of the brain*, terminating in a peculiar kind of suppuration, which the French call ramollissement. Lallmand thinks that inflammation is *always* the cause of this change of structure—Rastan, very wrongly thinks this softening of the brain to be the result of a peculiar process, which, however, he defines as little as Laennec, in his work, "*Sur l'Auscultation Mediate,*" does those of his "*ramollissement des tubercules des poudrons.*" This chronic inflammation of the brain, has hitherto been generally confounded with other diseases of the brain, especially with apoplexy; and it is therefore of much importance, that the diagnostic of this disease, be more particularly defined. This species of inflammation, is by no means uncommon; I have a case under my care at this very time. Of all those, however, which I have treated, I think I have perhaps cured but one. I say *perhaps*, for I consider the diagnostic of this terrible malady, still too obscure, to speak of it with any certainty. There is another work which I have lately received and read with very great satisfaction—It is a new work, by Lobstein—"Compte rendu à la faculté de Médecine de Strasburg sur l'état actuel de son Muséum anatomique—Par T. F. LOBSTEIN, Strasburg, 1820." I was particularly interested in the account of a conceptio extra-uterina, (i. e.) conceptio tubana, page 56; of which he says, "la grossesse dans le trompe de Fallope; la présence d'un fœtus dans l'épaisseur même des parois de la Matrice sans qu'on pût comprendre comment il y est parvenu." I have in my collection a similar preparation of a woman who died in this place, which, together with an account of the disease, and a case of conceptio tubana, I intend to publish in the Transactions of the Medico-Chirurgical Society of London. The woman last men-

tioned died in 10 hours, under the most terrible agonies; which were indeed excessively violent at first, but gradually abated, owing no doubt to the constantly increasing internal hæmorrhage. The cavity of the uterus, as in *most* cases, contained the decidua Hunteri; for in *some* cases this membrane has not been found.

"Among the more rare diseases, which have lately come before me, belongs a case of Pulegmasia alba dolens puerperarum, which I cured chiefly by means of vesicatories. I consider this disease as one of the neuroses. The grounds which have been offered in proof of its being an inflammatory disease, by the English and American physicians, I intend to controvert in a paper which I am about drawing up. I have also lately seen two cases of Eclampsia parturientium, one of which proved fatal, but the other was cured by large bleedings, which were also used, though ineffectually, in the first case. I have seen in all above 12 cases in my practice, and the number of those cured, exceed, thank God, more than the half."

LITERARY NOTICE.

THE Proprietor of the AMERICAN MEDICAL RECORDER is about to put to press AN ELEMENTARY SUMMARY OF PHYSIOLOGY, by F. MAGENDIE. Translated from the French, by a member of the Medico-Chirurgical Society of London, 2 vols. 8vo. with additions, by an eminent Practitioner.

☞ With a view to make the work as cheap as possible, the two volumes shall be printed in one volume octavo.

SURGICAL ESSAYS.

THE *Surgical Essays* by ASTLEY COOPER, F. R. S. Surgeon to Guy's Hospital, and BENJAMIN TRAVERS, F. R. S. Surgeon to St. Thomas' Hospital, *parts first and second, octavo*, from the last London edition, are in the press, and will be published in a few days with all the plates, *twenty-one in number*. This work shall be done in a manner not inferior to the London copy, and *cost at least one fourth less*.

*. * Gentlemen unacquainted with the character of the Surgical Essays, are referred to page 350 of this volume.

☞ THE Proprietor of the American Medical Recorder will furnish to order, any Medical (English) Books. *A list of late British Medical Publications may be seen in the supplement to the April number of this volume.*—See page 401.

ERRATA.

In page 460, line 10 from the bottom, for *nervous*, read *venous*.

461, line 20 from the top, for *two drops*, read *ten drops*.

461, line 22 from the top, for *two days*, read *ten days*.

620, the second, third, fourth, fifth and sixth lines from the top, read 6, 7, 8, 9, 10, 11, instead of 5, 6, 7, 8, 9, 10; and insert after the 4th article, or word "heat," on the second line from top of same page, 5. *That it diminishes the quantity of blood in the small vessels, but accumulates it in the large ones.*

620, line 24 from the top, for "*aere perennius*," read "*aere perennius*."

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
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